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EDWIN DUNKIN, F.R.S., President, in the Chair.

Samuel Palmer Chapman, 27 West Park, Clifton, Bristol; Duncan Forbes, 11 Hanover Buildings, Southampton; and Charles Horsley, F.G.S., 174 Highbury New Park, N.;

were balloted for and duly elected Fellows of the Society.

Notes on Nyrén's Determination of the Constant of Aberration. By David Gill, LL.D., F.R.S.

(*Extract from a letter to Professor Otto von Struve.*)

Nyrén's remarks on the possible systematic errors that may attach to determinations of the constant of aberration bring strongly to my recollection the course of thought which led me to write the article which you will find at pp. 40–43, vol. i. of "The Observatory."

I there mentioned that W. Struve's determinations of the constant of aberration exhibited indications of a term depending on the R.A. of the star observed. Nyrén's determinations also exhibit variations which are still more pronounced, and which can be very accurately expressed by terms depending on sin α and cos α—that is by sin $(M+\alpha)$; but the value of M would be quite different if it were deduced from his and from Struve's series.

Struve's series depends on too small a number of stars (notwithstanding the great accuracy of each result) to enable us to arrive at very accurate ideas as to the value of M; still, the fact that M is very different if deduced from Nyrén's series points to the probability that Struve's results (if Nyrén's are accurate) were affected by systematic changes in the clock rate or azimuth, which were more or less eliminated by the methods employed by Nyrén. I quite agree that the precautions adopted by Nyrén of comparing his clock before and after each set of observations with the normal clock (situated in air of uniform pressure, and

where the diurnal variation of temperature is insensible) enable us to dismiss from further consideration any errors on account of the clock.

But can we entirely dismiss errors depending on the azimuth? If I remember rightly, the marks employed and their lenses were not sheltered from the Sun quite in the same way as the azimuth mark of the Transit instrument; at least we have no detailed account of their construction.

Also, since azimuth marks in the prime vertical and the ground on which they are situated are exposed to greater causes of disturbance in azimuth than similar marks situated in the meridian, it is of the utmost importance that all care should be taken to obtain evidence of their freedom from diurnal variation.

[To explain further what I mean, the Pulkowa meridian marks and the soil on which they stand are exposed chiefly to the Sun from the south, which would tend to create their chief variations in the direction N. and S. (I speak of the diurnal effect as a whole), which, therefore, might tend to change their azimuth more in a N. and S. direction than E. and W.]

Now, as you have two and not four marks, you cannot from the marks alone separate the change of azimuth from change of collimation. But it would be of great interest to compare *separately* the azimuths of the two marks, employing the value of the collimation derived from the star observations, which collimation might be assumed to have a constant relation to the value of the collimation when the instrument is horizontal (of course you could not assume the collimation identical for the vertical and horizontal positions of the telescope). If by this means it could be shown that the relative azimuths of the marks show no diurnal variation of mutual azimuth, the determination as a whole, it seems to me, would have even a higher value —high as the present value, in my eyes, is. Of course I admit that the effect of any such systematic error is nearly (but not quite) eliminated by the distribution of the stars in R.A.; and I agree with Nyrén in the opinion that the definitive result of his researches is, on this account, more accurately estimated by giving equal weight to the results derived from stars in each quadrant of R.A., than by giving each separate result a weight corresponding to its probable error.

But if M has a real existence, it is a fact of the highest cosmical interest, and I think that no trouble should be spared to determine it with the highest possible accuracy.

If M were accurately determined at Pulkowa, and another value M^1 as accurately determined here, we should have from the two mean values of aberration and the values of M and M^1 the necessary data for determining whether or not the galaxy has a motion of translation in space, at least within certain limits; and I see no reason to suppose it impossible that that translation may amount to thirty or forty times that of the Sun's

translation relative to neighbouring stars—at least, I think Nyrén has dismissed the matter somewhat too absolutely. There is at least in his results very strong evidence of the existence of M, and this should be traced to its source.

With this in view I venture to suggest the following points beyond a further examination of the question of azimuth.

1. What are the parallaxes of the stars employed ?
2. What are the colours of the stars ?

1. The determination of the parallaxes of the stars employed in the research, relative to those of stars of 7th or 8th magnitude with a good heliometer, could be accomplished in two years with all desirable accuracy. I would write further on this point but that I have said all that I can say on this subject in the memoir which I mention in the accompanying letter, and which I hope to send you in a few months after I reach England.

2. If there be anything in Forbes's researches (which I am inclined to doubt on the evidence of Newcomb *), red and blue light have sensibly different velocities, and it would be very interesting to see what evidence Nyrén's investigations give on this point. If the redder stars give a smaller value for the constant of aberration than the whiter or bluer stars, it would be a fact of much importance.

Royal Observatory, Cape of Good Hope:
1883 *Dec.* 19.

Remarques sur les "Notes on Nyrén's Determination of the Constant of Aberration" de M. Gill. Par M. Nyrén.

Il m'a été de beaucoup d'intérêt de lire les remarques faites par M. Gill sur mon mémoire, "L'aberration des étoiles fixes." Quant aux doutes qu'il exprime sur quelques points du mémoire, je m'empresse de répondre ici en quelques mots.

Par rapport aux mires destinées à contrôler la constance en azimut de notre instrument du premier vertical, il a été dit dans mon mémoire que leur construction a été tout à fait analogue à celle des mires de la lunette méridienne. Les piliers portant les mires sont construits en briques et s'enfoncent de 10 pieds dans le sol, étant isolés du terrain environnant par un intervalle assez large. Les parties supérieures des piliers sont entourées deplanches, et l'espace entre les planches et les piliers est rempli d'étoupe de chanvre. Des maisonnettes en bois revêtues de mortier entourent l'ensemble de ces constructions. Autour de ces cabanes il y a des arbres en partie très vieux. Pour la mire d'Est, les arbres n'abritent pas complètement la maisonnette

* Newcomb said when here, that, with his powerful apparatus, if there had been a difference of $\frac{1}{1000}$ part in the velocity of red and blue light, his star (or spot of light) whose displacement, due the time of passage of light, he observed, would have been converted into a spectrum that could not have escaped his attention.

contre les rayons du Soleil; à l'Ouest, ce n'est qu'exceptionnellement qu'un rayon pénètre jusqu'à la maisonnette et le sol voisin. Les piliers dans la salle d'observation, portant les objectifs collimateurs, sont fondés de la même manière que ceux des instruments; ils sont toujours abrités contre l'insolation.

Avec ces précautions je le regardais comme assez sûr qu'aucune variation appréciable de la ligne de collimation de nos mires ne pouvait s'effectuer dans l'intervalle d'une ou de deux heures, entre les passages de la même étoile par les deux branches du premier vertical. Pour cette raison j'ai pensé que les détails des lectures des mires pourraient sans inconvénient être réservés à la publication des observations *in extenso*, procédé qui me parut d'autant plus justifié qu'avec les matériaux à notre disposition aucune sûreté absolue sur la variabilité dans les piliers des mires ne peut être gagnée. Cependant, les remarques de M. Gill me font croire qu'il serait peut-être opportun de donner un peu plus de détails sur cette question: je m'empresse donc de communiquer quelques résultats ultérieurs de mes études sur les lignes de collimation des mires et de l'instrument. Puisqu'il ne s'agit que des variations périodiques de ces lignes, je ne m'occuperai pas ici de leurs valeurs absolues.

Désignons, dans la position de l'instrument au Sud du pilier, par S_e et S_o les moments des passages d'une étoile par un fil du réticule dans les deux branches Est et Ouest du premier vertical; par N_e et N_o les moments des passages, observés le même jour, par le même fil, dans la position Nord. En supposant que l'erreur de collimation du tube et l'inclinaison de l'axe horizontal soient restées constantes entre les passages en Est et en Ouest, on devrait trouver dans nos observations:—

$$\frac{S_e + S_o}{2} - \frac{N_e + N_o}{2} = 0.$$

En admettant que, dans l'intervalle entre les passages par les deux branches du premier vertical, l'erreur de collimation ait changé de la quantité dc, on aura approximativement:—

$$dc = \sin \phi \cos \delta \sin t \, . \, dt,$$

où

ϕ = latitude

δ = déclinaison de l'étoile,

t = angle horaire pour le passage par le pr. vert.

$$dt = \frac{S_e + S_o}{2} - \frac{N_e + N_o}{2}$$

Une valeur positive de dt indiquera que l'angle formé par l'axe horizontal et l'axe optique du côté de l'oculaire du tube a augmenté.

Les dc déduits par cette voie de toutes nos observations ont été comparés avec les variations dm, fournies directement par les lectures des mires faites immédiatement avant et après les

observations. Pour les différents quadrants en ascens. droite on a trouvé ainsi en moyenne :—

		Matin.	Soir.
h h		"	"
0–6	$dc-dm$	$=-0.24$	$+0.04$
6–12	,,	-0.28	-0.05
12–18	,,	-0.06	-0.05
18–24	,,	-0.15	-0.19

Si l'on voulait attribuer ces différences à des variations de la ligne de collimation des mires, ce seraient les différences Matin-Soir dont on devrait tenir compte dans la déduction de l'aberration. On aurait donc :—

		Matin—Soir.		Décl. moy. des étoiles observées.
h h		"	"	° ′
0–6	$dc-dm$	$=-0.28$	± 0.058	59 10
6–12		-0.23	0.065	59 13
12–18	,,	-0.01	0.064	59 9
18–24	,,	$+0.04$	0.060	59 1
Moyenne ...	...	-0.120	± 0.031	59 8

Les observations étant réduites dans la supposition que les mires soient restées immobiles entre les passages Est et Ouest, il serait donc possible que les observations faites aux environs du maximum (ou minimum) de l'aberration exigeraient encore une correction moyenne de $0''.006$, correction de beaucoup inférieure à l'err. pr. $\pm 0''.015$, trouvée pour le résultat de notre dernière série d'observations. Mais tant que nous ignorons dans quelle mire la variation ait eu lieu, ou, ce qui revient au même, lorsqu'on ne veut admettre que l'instrument soit resté invariable en azimut, nous ne pouvons même dire si cette correction devait être positive ou négative.

Lorsqu'on compare, comme le propose M. Gill, les quantités dc avec les dm déduits de chaque mire séparément, on trouve comme variations horaires pour les différents mois :—

		Est.		Ouest.	
		Matin.	Soir.	Matin.	Soir.
		"	"	"	"
Janvier	$dc-dm$	$=-0.07$	$+0.13$	-0.15	-0.13
Février	,,	0.00	$+0.02$	0.00	-0.02
Mars	,,	$+0.11$	$+0.18$	-0.19	-0.14
Avril	,,	-0.11	-0.01	-0.15	-0.35
Mai	,,	$+0.15$	$+0.12$	-0.01	-0.52
Juin	,,	-0.38	$+0.15$	-0.56	-0.39
Juillet	,,	$+0.01$	-0.08	-0.29	-0.06
Août	,,	-0.02	$+0.10$	-0.26	-0.16
Septembre	,,	$+0.05$	-0.04	-0.23	-0.14
Octobre	,,	$+0.02$	$+0.25$	-0.16	-0.31
Novembre	,,	-0.21	-0.27	-0.53	-0.31
Décembre	,,	—	-0.37	—	-0.01

En apparence les valeurs plus fortes de ces différences pour la mire Ouest devraient conduire à la conclusion que cette mire, plus que l'autre, ait été sujette à des variations de position, ce qui paraît peu probable, vu qu'elle est si bien abritée contre les rayons du Soleil. Mais au lieu d'attribuer les écarts à une cause si peu probable, et dont l'existence n'est pas démontrée, il paraît plus raisonnable de se tenir à des explications basées sur des phénomènes déjà bien constatés. Comme tels phénomènes il faut mentionner ici :—

1°. Variations de l'instrument en azimut, distinctement prononcées dans les lectures des mires.

2°. Différences systématiques entre les *dc* déduits de la manière expliquée précédemment, et les variations réelles de l'erreur de collimation de l'instrument. La vérité de cet énoncé est démontrée par le fait que les valeurs moyennes des *dc*, déduites des observations commencées avec l'instrument Sud du pilier, ne s'accordent pas avec les *dc* fournis par les observations commencées Nord. En combinant les *dc*, pour chaque quadrant, en deux groupes selon la position primitive de l'instrument, on trouve :—

	Matin.			Soir.		
	dc_S	dc_N	$dc_S - dc_N$	dc_S	dc_N	$dc_S - dc_N$
h h	"	"	"	"	"	"
0–6	−0·49	−0·10	−0·39	+0·07	+0·11	−0·04
6–12	−0·52	−0·05	−0·47	+0·13	+0·21	−0·08
12–18	−0·18	−0·06	−0·12	+0·18	+0·40	−0·22
18–24	−0·36	+0·04	−0·40	−0·19	+0·10	−0·29

Puisqu'il est inadmissible de supposer que les variations réelles de l'erreur de collimation soient différentes pour les deux moitiés du réticule, les écarts $dc_S - dc_N$ portent à la conclusion que ni l'une ni l'autre valeur de *dc* ne coïncide avec la variation réelle de la dite erreur, et, par conséquent, que rien ne s'oppose à la supposition que cette variation soit en réalité exprimée par les valeurs trouvées de *dm*.

L'explication des différences notées doit être cherchée, sans doute, dans l'équation personnelle de l'observateur concernant les passages par les fils du réticule. Pour rendre cette explication plus probable, il suffit de mentionner que la dite équation peut varier par plusieurs causes : par la vitesse du mouvement de l'étoile, par la direction de ce mouvement, par la position de l'observateur, tous changements qui arrivent dans chacune de ces observations, mais qui se combinent de différentes manières si l'observation est commencée au Sud ou au Nord du pilier. Les différences des valeurs $dc_S - dc_N$ pour les différents quadrants pourraient bien être accidentelles.

M. O. Struve a trouvé pour les variations de la collimation produites par des changements de la température, la valeur +0·″087 pour −1° R. (Obs. de Poulk., vol. iii. p. 201), et presque la même

valeur résulte de notre série d'observations si on la déduit de la même manière. La prépondérance du signe positif pour les valeurs de dc, déduites des observations du soir, est donc en bon accord avec cet effet de la température. Mais aussi pour les observations du matin on devait attendre le même signe, vu que la température s'est abaissée en 200 cas environ, et n'a monté qu'en 50. Mais comme on voit, l'effet des autres causes mentionnées a prévalu pour cette époque du jour. On retrouve un phénomène analogue dans la série de W. Struve. En déduisant les dc, pour les deux époques du jour, des observations isolées—non par la comparaison d'observations différentes, instituées en différentes saisons—on trouve pour dc_m (matin)—dc_s (soir), et les changements correspondants de la température v_m (matin)—v_s (soir), les valeurs moyennes :—

	$dc_m - dc_s$	$v_m - v_s$
	″	°
β Cassiop.	−5·95	+2·71 R.
δ Cassiop.	−1·72	+1·57
ι Dracon.	−1·61	+0·07
b Dracon.	−1·95	+1·33
o Dracon.	−3·15	+0·98
Ceph. 2 H.	−1·35	+2·21

On voit donc qu'aussi dans cette série les variations $v_m - v$ ne suffisent pas pour expliquer les différences $dc_m - dc_s$.

Il se présente maintenant la question très importante si les variations supposées de l'équation personnelle ont pu exercer une influence sensible sur les distances zénithales observées. S'il en était ainsi, on pourrait attendre que les observations commencées au Sud du pilier donneraient en moyenne d'autres résultats que dans le cas où l'on a commencé au Nord. C'est pour diminuer l'incertitude qui en résulterait que, presque sans exception, on a suivi la règle de commencer les observations alternativement au Sud et au Nord. En combinant, sous ce point de vue, toutes les différences en déclinaison : observation—calcul, en deux groupes, on trouve l'écart au commencement Sud—commencement Nord :—

		Matin.	Soir.
h h		″	″
0–6	S−N	= +0·034	+0·055
6–12	,,	+0·011	+0·002
12–18	,,	+0·003	+0·021
18–24	,,	+0·033	+0·077

Il s'ensuit qu'aussi sans cette précaution, le résultat pour l'aberration aurait été presque identiquement le même. Ce fait nous permet de conclure que les variations supposées dans l'équation personnelle s'éliminent à peu près complètement dans les observations isolées.

Concernant la remarque de M. Gill sur le point de mon mémoire, où j'exprime des doutes sérieux sur la possibilité de parvenir à la connaissance du mouvement absolu de translation du système galactique par des déterminations de la constante de l'aberration en différents endroits du ciel, c'est une question qu'on ne peut, *à priori*, ni démontrer ni nier absolument. Je me bornerai donc à dire, qu'entre les différentes explications qu'on pourrait proposer pour rendre compte des différences entre les valeurs de l'aberration trouvées dans notre série, par des étoiles de différentes ascensions droites, ce ne serait qu'avec la plus grande précaution qu'on puisse accepter celle qui voudrait attribuer les dites différences au mouvement du système galactique. Il me paraît plus probable que les saisons y ont joué un rôle important.

Note to Accompany Correspondence on the Subject of Nyrén's Determination of the Constant of Aberration. By David Gill, LL.D., F.R.S., Her Majesty's Astronomer at the Cape of Good Hope.

Nyrén's recent determination of the constant of aberration with the Prime Vertical Transit Instrument at Pulkowa, and his discussion of the values of the same constant as determined with the Transit Instrument and Vertical Circle of that Observatory, together constitute one of the most interesting and valuable contributions to modern astronomy.

The wonderful agreement of the results obtained by the three instruments in question, from different stars and totally different modes of observation, affords the most striking testimony not only to the genius of the elder Struve, who planned the instruments and methods of work, but also to the zeal and ability of the astronomers who have so efficiently carried on and refined the traditions of high accuracy that have always been associated with the work of Pulkowa. In writing on these and other matters to Otto von Struve I was anxious to obtain further information as to certain points in M. Nyrén's work, because it seemed not improbable that germs of results even still more interesting might be found in it. I accordingly enclosed in my letter the above notes for M. Nyrén's perusal, but without any intention of publishing them in their present form. In reply to these notes M. Nyrén has made some further investigations, the results of which he has kindly communicated to me, and they are so interesting that (with M. Nyrén's consent) I forward them for publication in the *Monthly Notices*.

Observations of Comet a 1884, *made at Windsor, New South Wales.*
By John Tebbutt.

On the evening of January 14 I received from the Melbourne Observatory a telegram to the effect that a small comet was found there on the 11th. Its position and daily motion for the 12th were communicated, but owing to clouds and smoke I did not succeed in finding the comet till the 19th. It was then just beyond the reach of unassisted vision. In the telescope it presented itself as a round nebula, about two minutes of arc in diameter, with a bright condensation in the centre. There was no definite nucleus in the micrometer eye-piece, but a short tail could be distinguished. The comet faded very rapidly, and was observed with extreme difficulty on February 2. The accompanying places have been deduced from comparisons made with a square bar micrometer on the 4½-inch equatorial. In bringing up the mean places of the comparison stars from the various authorities the precessions and secular variations have been adopted from Stone's Cape Catalogue. With the exception of star No. 6 no reliable authority exists for proper motion. The proper motion of this star is very large, and has been taken from that Catalogue. I trust at a future opportunity to be able to send a determination of the orbit. Considering the rapid fading of the comet from our view it is probable that the heliocentric motion is retrograde. I commenced observations of Pons' comet on January 13.

Apparent Places of Comet 1884.

Windsor Mean Time. 1884.					R.A.			Log. $\frac{p}{\Delta}$	N.P.D.			Log. $\frac{q}{\Delta}$	No. of Comps.	Comp. Star.
	d	h	m	s	h	m	s	+	°	′	″	+		
Jan.	19	9	37	42	22	54	35·76	8·8669	131	46	56·3	9·6972	6	1
	19	9	37	42	22	54	35·55	8·8669	131	46	56·5	9·6972	6	2
	21	9	26	7	23	5	2 58	8·8712	131	54	59·2	9·6657	4	3
	21	9	31	39	23	5	3·91	8·8701	131	55	2·7	9·6781	5	4
	22	9	3	4	23	9	44·99	8·8735	131	57	21·3	9·6073	8	3
	22	9	3	4	23	9	44·93	8 8735	131	57	19·5	9·6073	8	4
	23	9	22	11	23	14	18·89	8·8726	131	58	45·3	9·6531	5	3
	23	9	22	11	23	14	18·79	8 8726	131	58	43·8	9·6531	5	4
	24	8	51	49	23	18	29·18	8·8729	131	59	15·1	9·5742	5	3
	24	8	51	49	23	18	29·05	8·8729	131	59	12·6	9·5742	5	4
	25	9	14	35	23	22	34·15	8·8734	131	59	19·6	9·6338	1	3
	25	9	14	35	23	22	34·13	8·8734	131	59	18·7	9·6338	1	4
	27	8	50	59	23	29	57·41	8·8727	131	57	33·4	9·5731	2	3
	27	8	50	59	23	29	57·32	8 8727	131	57	31·6	9·5731	2	4
	28	8	56	48	23	33	26·24	8·8731	131	55	54·1	9·5907	4	5
	28	8	56	48	23	33	26·07	8·8731	131	55	50·9	9·5907	4	6
Feb.	2	9	38	9	23	48	48·21	8·8661	131	44	19·1	9·7002	3	7

Mean Places of the Comparison Stars for 1884·0, *with the Reductions to the Apparent Places for the Dates of Observation.*

Star.	Mean R.A.	Reduction.	Mean N.P.D.	Reduction.	Authority for Star's Mean Place.
	h m s	s	° ′ ″	″	
1	22 49 17·36	−0·93	131 42 57·1	+5·4	Cordoba Zone 29, No. 51; Cape Cat. 1880, No. 11941.
2	22 53 50·84	−0·92	131 46 18·8	+5·7	Wash. Cat. 1860, 2nd ed., No. 10109; Cordoba Zone 29, No. 53; Cape Cat. 1880, No. 11966.
3	23 8 32·22	−0·88	131 44 0·2	+6·5	Wash. Cat. 1860, 2nd ed., No. 10232; Melb. Cat. 1870, No. 1183; Cordoba Zone 29, No. 66; Cape Cat. 1880, No. 12062.
4	23 10 25·93	−0·87	131 49 39·8	+6·6	Wash. Cat. 1860, 2nd ed., No. 10249; Cordoba Zone 29, No. 67; Cape Cat. 1880, No. 12080.
3	23 8 32·22	−0·89	131 44 0·2	+6·4	See authorities above.
4	23 10 25·93	−0·88	131 49 39·8	+6·5	„ „
3	23 8 32·22	−0·90	131 44 0·2	+6·2	„ „
4	23 10 25·93	−0·89	131 49 39·8	+6·4	„ „
3	23 8 32·22	−0·91	131 44 0·2	+6·1	„ „
4	23 10 25·93	−0·90	131 49 39·8	+6·2	„ „
3	23 8 32·22	−0·92	131 44 0·2	+6·0	„ „
4	23 10 25·93	−0·92	131 49 39·8	+6·1	„ „
3	23 8 32·22	−0·94	131 44 0·2	+5·7	„ „
4	23 10 25·93	−0·94	131 49 39·8	+5·8	„ „
5	23 31 50·93	−0·88	132 12 28·1	+7·3	Cordoba Zone 51, No. 25; Cape Cat. 1880, No. 12222.
6	23 40 23·59	−0·84	132 11 53·8	+7·9	Melb. Obs. 1874, No. 404; Melb. Obs. 1875, No. 341; Cordoba Zone 51, No. 37; Cape Cat. 1880, No. 12277.
7	23 45 39·68	−0·85	131 28 14·3	+7·5	Cordoba Zone 51, No. 46; Cape Cat. 1880, No. 12319.

Windsor, N. S. Wales,
February 12, 1884.

Observations of Comet Ross made at the Observatory, Melbourne. By R. L. J. Ellery, F.R.S.

Date, Melb. M.T. 1884.				Δα	Δδ	App. α.	App. δ. South.	Comparison Stars. α	Comparison Stars. δ	Authority.
d	h	m	s	m s	′ ″	h m s	° ′ ″	h m s	° ′ ″	
Jan. 12	9	3	29	+4 31·0	+ 0 5·6	22 3 37·0	40 7 14	21 59 6·0	40 6 8·4	Cape Cat. 11603
17	9	16	31	+4 28·3	+ 7 19·6	22 42 35·2	41 30 57	22 38 6·9	41 22 37·4	Cape Cat. 11850
18	8	31	25	+0 38·0	− 2 42·0	22 48 38·5	41 40 22	22 49 16·5	41 43 3·9	Cape Cat. 11941
28	10	41	30	+1 0·0	−11 15·1	23 33 45·0	41 55 54	23 32 45·0	42 7 9·1	Washington 10436
29	9	29	3	−3 29·6	−17 46·4	23 36 53·0	41 54 10	23 40 22·6	42 11 56·4	Cape Cat. 12277
Feb. 1	9	25	2	+0 18·3	+15 27·7	23 45 57·1	41 43 47	23 45 38·6	41 28 19·3	Cape Cat. 12319
4	9	24	0	−0 24·5	+18 12·3	23 54 9·0	41 38 26	23 54 33·7	41 20 13·7	Cape Cat. 12395
7	8	44	31	−2 16·2	− 2 32·0	—	—	0 3 38	41 33 47	Anonymous

Cloudy weather came on after the 7th, and there was no really clear sky on any evening until the 19th, when a very rough measure was obtained viz. :—

R.A. ... h m s 0 25 50 Decl. ... ° ′ ″ 40 56 0 S.

The comet was then so faint as to be seen with difficulty, and no glimpse of it has been obtained since, owing to continual cloudy evenings. It was rapidly increasing its geocentric distance from January 12.

On a New Dark Field Micrometer, and on the Electric Illumination of an Equatorial at Melbourne. By R. L. J. Ellery, F.R.S.

A new dark field micrometer has been lately made for the 8-inch Equatorial of the Melbourne Observatory, which presents some novel features, and which has been found in practice to be very convenient and remarkably useful in micrometric observations of faint objects. It is arranged to be used either with "dark" or "bright field" illumination; and in the "dark field" means are provided for illuminating the declination or micrometer webs, or the Right Ascension or Transit webs, separately or together. The light is given by a small incandescent lamp of $2\frac{1}{2}$-candle power, which with a rheostat can be conveniently graduated from a simple glow to the light of two candles, while the light can be sifted by means of coloured glass, light ruby being generally used.

The diagrams will show the arrangement.

The micrometer is of the ordinary modern form, one slide, S′ (fig. 1), is movable by means of the micrometer screw (100 threads to the inch), with fixed divided head, h^1, and a count head, h^2 (fig. 2), actuated by epicyclical tram within the head. This slide carries three webs about 6′ apart. The second slide, S″, carried five webs about 4′ 48″ apart, and has a very limited motion by means of a screw, k (figs. 1 and 2), with 100 threads to the inch, which can be moved only with a special key. The whole micrometer box is moveable at right angles to the optical axes of the telescope through being attached to the adapting tube by a well-fitting slide, $b\,b$ (fig. 1), actuated by the screw s^3, which is opposed by a strong spiral spring c (fig. 1). The eye-pieces are mounted on a slipping-piece moved by the four threaded screws, S^4 (fig. 2).

The illumination.—The adapting tube, T (fig. 2) is $2\frac{1}{2}$ inches diameter and $4\frac{1}{2}$ inches long; at right angles to this is fixed a short tube, T^2, $2\frac{1}{2}''$ diameter and $2''$ inches long, to which is fixed the electric lamp, L (fig. 1 and 2), from which light diverges on to lenses l^1 and l^2 (fig. 2), and thence parallelised through a perforated screen, and coloured glass if required, on to four small mirrors, $m\,m\,m\,m$ (fig. 2), fixed, but adjustable, on a diagonal diaphragm. These mirrors reflect four beams up the annular space between the adaptor tube and a smaller inner tube, $t\,t$ (fig. 2), toward the micrometer box, and exactly upon four small rectangular apertures shown at $a\,a$ (fig. 1), thence through them to four small mirrors, two of which are seen in fig. 2, $d\,d$, which reflect a thin beam of light a little from above, but almost precisely in the plane of the wires, giving them a full and symmetrical illumination. To admit of this, the surfaces on which the webs are laid are below the general surface of the slides, which are cut away for the purpose, as shown in fig. 1. In the adapting tube and close to the bisecting slide are four very light shutters, $f\,f\,f\,f$ (fig. 3), which can be closed over either or

SECTION THROUGH AB. (Fig. II.)

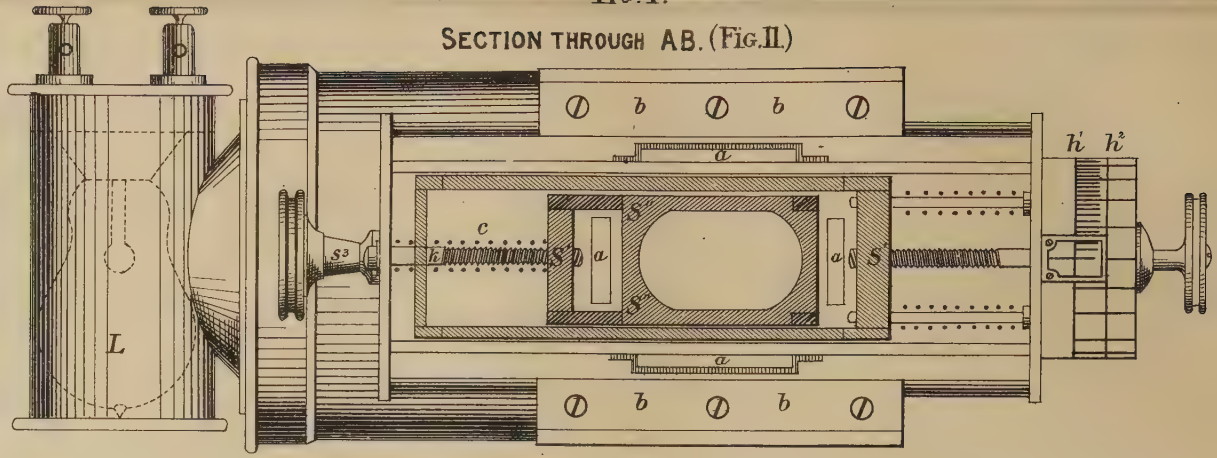
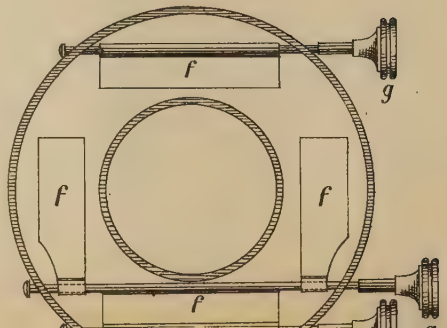


Fig. III.

SHUTTER APPARATUS.



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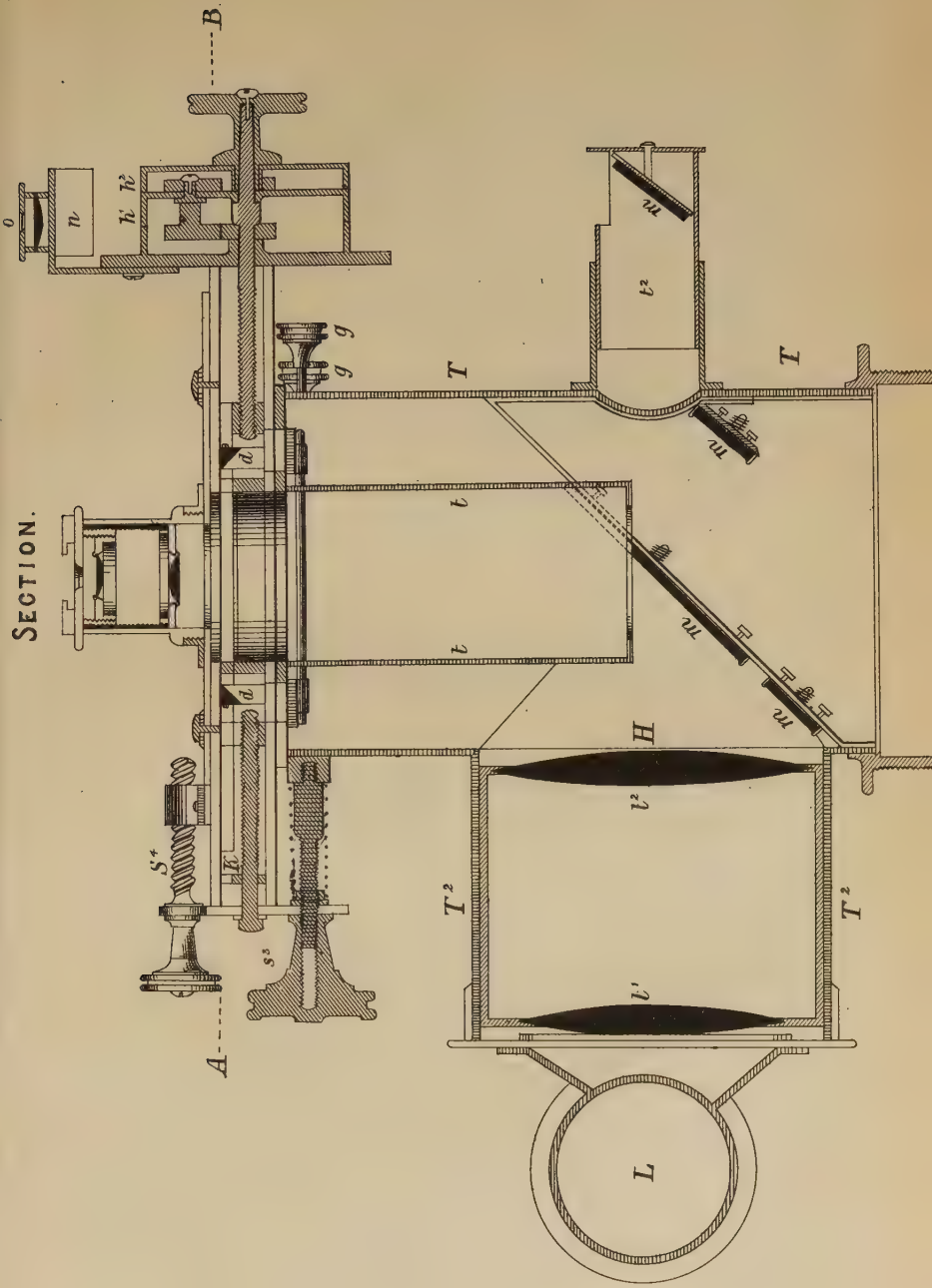

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both pair of openings by means of little milled heads, *g g g* (figs. 2 and 3) behind the micrometer box, so that if required either or both systems of wires can be eclipsed or both illuminated. A further advantage is afforded by this part of the apparatus, which is often an advantage in observing faint objects: the declination or micrometer wires proper can be *half eclipsed*, so that the position of an object may be got by pointing the end of the illuminated wire (which is mid-field) to the object. Objects that are invisible behind a faintly illuminated wire can be thus plainly seen. Of course, by shutting off the lamp L, and taking illumination from the ordinary central mirror of the telescope, a bright field with dark wires results.

In micrometer work it is often very inconvenient to be obliged to hold a lamp to read off the micrometer head, and to obviate this the following arrangement is made. In the screen at H (fig. 2) is a central aperture which allows a beam to pass into the tube t^2 (fig. 2), carrying a small mirror *m*, which reflects the light into another small mirror (not seen in section), thence to a dead white surface at *n* (fig. 2), giving a nice illumination of the divided heads, which can be read comfortably and conveniently with the little lens *o* (fig. 2). This lens folds back, to be out of the way of the face when using high powers.

There is, however, one inconvenience in this form of micrometer: the distance from the plane of the webs to the plane of the eye-piece slide makes it necessary to bring the eye very close to the plate for the highest powers, but this inconvenience is only relative and presents no absolute difficulty.

As regards the electrical illumination of the telescope, there are five incandescent lamps used in divided circuit, one for the micrometer, as described, one for the declination setting circle, one for microscopes of declination circle, one for central illumination and for position circle, and one for illumination of hour circle microscopes and reader. All these are connected with a small commutator and rheostat, fixed to the declination sector, with a small ebonite handle easily reached while observing. With this arrangement, by turning the handle, any one of the lamps is put in circuit, and the amount of light regulated by pulling or pushing a light-sliding tube on the commutator handle.

The current for the lamps is obtained from a battery of four carbon-zinc elements in bichromate fluid and cells, arranged in cascade. As this form of battery is found very successful for the purpose, it may be useful to describe the arrangement.

Four carbon-zinc elements are mounted on a frame, each set easily removable. This frame is suspended and counterpoised over four large glass cells, arranged in steps, so that when the fluid is 6′ high in the first cell it overflows into the second, and so on, and from the fourth it overflows into a portable vessel. The source of fluid supply is a lead-lined box of about four gallons capacity, to which is fitted a glass stop-cock for regu-

lating the flow. The square surface immersed of each element is about six square inches, and the delivery of fluid from two to four quarts per hour, when a very constant and steady light is given. When not in use the elements are lifted out of the cells and the flow of the fluid stopped. The fluid requires renewing as soon as all warmth of colour by transmitted light disappears, but it will produce a steady light until every bit of bichromate is decomposed; by simply increasing the flow as the fluid becomes weaker it will do a great many hours' work before it requires renewing.

Suggestions for Improvements in the Construction of Large Transit Circles. By A. A. Common.

The ordinary well-known form of Transit instrument, considered in regard to the purpose for which it is used, may be taken as the best designed astronomical instrument we have, fulfilling as it does, when of moderate dimensions, nearly all the required conditions. Its chief defect, inherent in the construction usually adopted, is that of flexure of the tube, and in a less degree of the axis itself. If this flexure of the tube is equal on both sides the result is only a slight movement of the optical axis parallel to itself, but if it is unequal then a change of direction necessarily follows. In either case the accuracy of the observation is not affected.

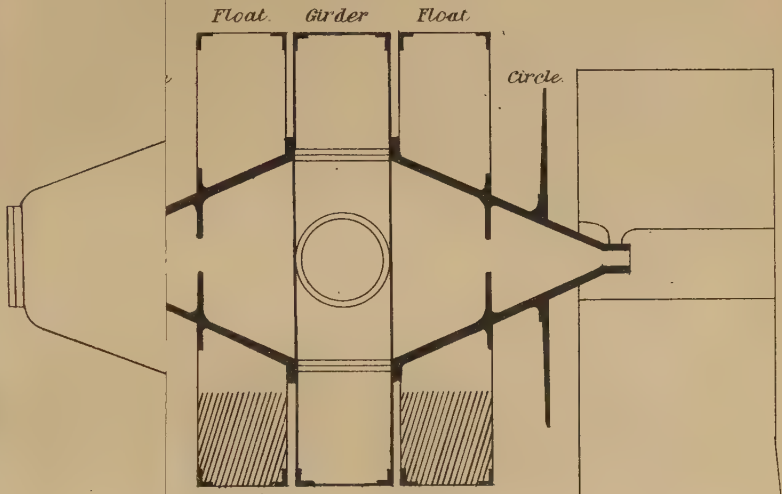
Most probably such permitted flexure would result in some change of direction of the optical axis in a plane not at right angles to the axis; but, at the worst, this resulting error is all that the Transit instrument suffers from.

When, however, this instrument, good as it may be for its own purpose, is made into a Transit Circle by placing on the axis a divided circle for the purpose of determining the direction of the optical axis in a vertical plane, without in any way altering the construction to get rid of the defect spoken of, that would here become of great importance, it cannot be said that all has been done to make it as perfect or as suitable for its purpose as possible, especially as the delicacy and refinement of observation require that the means of the astronomer should be as mechanically perfect as they can be made, so as to reduce to the lowest amount those errors that must always exist in an astronomical instrument.

Believing, as I do, that the Transit Circle, as at present made, is capable of great improvement, I wish to lay before the Society some remarks on the construction usually followed, and to offer some suggestions for consideration.

If we examine the construction of this instrument we find that, generally speaking, it is only an enlargement of the

ten feet focal length.



Section through Axis.
Troughs not shewn.

ordinary Transit instrument, with the addition of a divided circle and means to read it, and that there is not any real change of plan or evidence of any attempt to meet the altered conditions of the case. Rigidity is expected from increase of metal; but, as I shall show presently, this increase is, in most cases, not so well disposed as in the model. The usual construction is something like this. There is a hollow metal cube, from two sides of which spring the cones, terminating in the pivots; this forms what may be termed the axis; to the other two opposite sides of the cube are attached, by flanges, the tubes carrying the optical points of the instrument. The remaining sides of the cube are perforated for the use of the collimators. The connection of the tubes to the cube by flanges and bolts is general, and raises the question as to the effect of bolts if screwed up unequally.

Now, if we take a section of such an axis, we find that the metal does not, at the angles formed by the cube, cover the lines of compression or tension, as the case may be, but is outside these lines—to my mind as fatal a disposition as can be imagined for such a case. If we place such an instrument with the tube horizontal what do we find takes place as the result of gravity? The hole in the upper face of the cube becomes elliptical with the major axis in the direction of the tube, for the tension of the upper sides of the tube tend to pull it so, assisted by the compression of the upper parts of the cones at right angles to this direction, the reverse action going on below; and, further, the elbows formed by the corners of the cube and the junction of the flanges with the tubes alter their angle under the stress, and the consequence is flexure, principally in the plane at right angles to the length of axis, but not either equal or confined to this plane, and necessarily of a very uncertain character, and in large heavy instruments of considerable amount. The flexure that would, in a small instrument, be lost sight of, not only from its small amount, but from the want of power in such an instrument to detect it, becomes, in a large instrument, greater in proportion to the size, and more evident in proportion to the increased power.

The connection of the divided circle (and here I would like to say that in my opinion the making of circles like wheels, that is with spokes and a rim, is wrong altogether) by which the zenith distance of the optical axis is determined is not of such a nature as affords the best results, nor can it be where the flexure of the tube is admitted, and the rigidity of the circle is presumed. Properly, there should not be any more possibility of change in one more than in the other.

The counterpoise friction wheels are usually applied near the pivots, and not, as they certainly should be, as near as possible to the centre of gravity of each half of the axis; and are all of a mechanical character, that is, wheels pressed up by weights and springs.

The pivots invariably work in Ys, either with flat faces or concave for a certain amount to fit the pivots, and oil is used between the surfaces.

In making an observation, as far as I know, the determination in declination is made by bringing the fiducial wire as near the star as possible, and determining the exact position of this fiducial wire, representing the optical axis of the telescope, by reading many microscopes, and then measuring the distance of the star from this fiducial wire by the eyepiece microscope.

There are other matters, more of detail but not less important, well worth remark, but as they are not affected by the alterations I go to propose, they need not now be touched upon.

The symmetrical form of the Transit instrument has so many advantages over any modification of the mural circle form (and there are many that suggest themselves on thinking over this subject), particularly in the power of reversal, that it should certainly be retained. Keeping as near as may be to it, I would suggest the following alterations from the plan already mentioned. (Without going too minutely into details that could only be properly dealt with by working drawings, I have endeavoured to show these at a glance in the accompanying sketches, giving farther on some figures to show size and weight.)

The main axis should be, as in the Transit instrument, of two cones, but these I would connect base to base by an overlapping ring-joint. This joint would be cut in four places to allow for the light from telescope and collimators to pass. To this axis, in place of the usual tube, I would attach two thin discs of metal, about one-tenth less in diameter than the focal length of object-glass, and at a distance apart a little more than its aperture. In the case of the focal length exceeding a moderate amount—say six or seven feet—I would use, in place of the discs, sheets shaped somewhat in the manner shown in the sketches.

These discs or sheets are to be connected by an outside band and radial plates, and to be securely fastened to the axis already named by flanges in a vertical plane.

These discs, bands, and radial plates form practically a box-girder, the depth of which in the smaller size is equal to its length, and in the larger sizes is some fraction not less than one-half.

For the purpose of supporting these discs or sheets and the axis, in place of the usual mechanical counterpoise, I would have two short hollow iron cylinders immersed in their lower part in troughs of mercury to such an amount as to give the requisite lifting power, which in this construction would be slightly greater than the whole moving mass. These cylinders would be rigidly connected to the axis and the discs or sheets, and they would be placed so as to be as near as possible to the centre of gravity of each half, to reduce all strain on the central parts as much as possible. These floating counterpoises, while not so neat-looking as the mechanical ones,

have so many advantages in their freedom from vibration, their delicacy and constancy, and above all their perfection as reducers of friction, that they are quite worth the sacrifice to appearance their use demands.

The troughs for the floats to work in would be supported from below by suitable means to allow for the lowering and raising, running out, reversal, and return of the instrument.

The divided circles may be, in the case of a moderate-sized instrument, the discs themselves, or they may be, as is usual and more convenient, fastened to the axis near the pivots. In any case the circles should have a lenticular section, or be like a plate dished out a little, and not any kind of wheel with spokes and rim.

The pivots and their supports, determining as they do the absolute position of the instrument, are of the first importance. In place of the usual Ys, I would suggest that plane surfaces be used, one horizontal and one vertical, for each pivot bearing; these planes, of some hard stone, such as black carbonate, embedded in metal blocks, and brought to a true plane surface, the pivots being kept up to contact with the horizontal plane, and not, as is usual, brought down upon it; the contact of the pivot with the vertical plane being by any suitable means, as, for instance, a slight spring or a small pressure-wheel, exerting sufficient force to overcome any tendency of the pivot to roll instead of slide on the plane when the instrument is turned. In such a case the whole of the base parts supporting the troughs would have to be counterpoised by gravity, so as to have the requisite pressure to ensure contact between pivot and plane. It will be seen that in this arrangement the duty of the pivots is only to fix the plane of rotation of the instrument, and that they can never be subject to the whole weight of the moving part, but only to the amount of pressure spoken of, that is, sufficient to ensure contact; they are in this way relieved from the possibility of undue strain, the whole weight being always borne by the floats even in the reversal. The finishing of the pivots would be done after the instrument is permanently set up.

In the ordinary form the pivots have not only to do this duty spoken of, but they have to support more or less of the whole weight, assisted by the counterpoises, but always with the probability that at some time this whole weight may be brought on them, while in lifting or reversal, the pressure is generally applied at some fresh part of axis, so bringing new strains to work. It is doubtful if such an instrument would not be permanently deformed if once left to support itself on the pivots.

The fixing of the object-glass in its cell, and the preservation of the optical axis, that is, the line joining the optical centre of object-glass and fiducial lines in eye end, from change due to alteration of temperature, would seem to be best met by engraving these lines on a piece of glass similar in size to the

object-glass, and held, as regards its power of expansion or contraction, or the effect of this on the holder, in the same way as the object-glass. Such lines ruled on glass would be in every respect better than spider web, as not having any flexure, whilst being much finer. The use of graphite instead of oil as a lubricant would seem to have advantages in all cases, but more particularly where there is some little surface of contact, as when the Ys are made concave to suit the pivots, and would be well worth trial in existing instruments.

The use of an ordinary level would be impossible owing to the great size of the central parts between the pivots, but much might be done to get over this difficulty by using a column of mercury under each pivot, connecting the two columns below; in fact, using a U-shaped tube of a suitable width, filled with mercury, and provided with some means of causing the mercury to rise at will so as to show convex above the tops of the tubes. On placing this level so that each tube comes within about one-tenth of an inch of the underside of pivot, and applying pressure, the rise of the mercury could be watched, and the level of the two pivots got fairly well. If such a level had at the lower part of the U an enlargement in which could be placed a vertical septum of some flexible but not elastic material, that would serve as an insulator to keep the two columns electrically separate without affecting them otherwise, the contact of each column could be determined in a very exact way by a very weak battery and a fine galvanometer placed in circuit with the pivot and column of mercury, the contacts taking place at each pivot and showing on the galvanometers at the same instant if the pivots are level, when the mercury is slowly forced up. Such a level could be used with the ordinary form of Y, and in either case could be a permanent fitting. The pivots would have to be free from oil, of course, to ensure electrical contact, but the use of graphite would be favourable to this plan.

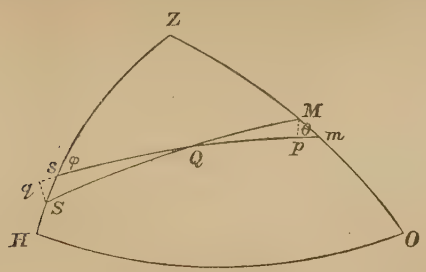
Taking the sketches given to roughly represent the moving parts of a Transit Circle of ten inches aperture, and ten feet focal length, with an axis six feet long, with a box-girder in place of discs to carry the optical parts, and provided with two circles of solid steel about three feet six inches in diameter, and allowing one hundred pounds for object-glass and eye-end fittings, we have the following approximate weights of the moving part, all in steel:—

	lbs.
The axis of cast steel 30″ diameter in two lengths with flanges	1100
The central part of steel one-fifth inch thick, including all angles, partitions, and fittings	1050
Two cylinders 56″ diameter, one-eighth metal, including all angles, etc.	700
Two circles, 42 diameter	350
Clamping gear	150
	3350

This is not an excessive weight for such a large instrument, and it is rather a question whether it might not be increased considerably. The displacement of about five cubic feet of mercury would equal it, and the cylinders of a length of one foot would give this amount of displacement with a margin. There are no mechanical difficulties in such a construction, though there are points that would require careful consideration. It allows of all the corrections and adjustments that the present instrument allows, and in the case of a small instrument where discs are used in place of what I have termed the box-girder; if required, more than one telescope, that is O. G. and eye-piece, could be used; and even a double telescope, at right angles to the principal telescope, that is, one where the O. G. of one is in front of the eye-piece of the other, and the lines of reference can be engraved on the object-glass of each; or one at some required angle from the first, for differential observations. The friction being so reduced the setting of the instrument actually on the star to be observed could be easily effected with proper clamping and moving arrangements.

A Method for Clearing a Lunar Distance. By John Merrifield.

Let Z be the zenith, HO the horizon, m and s the apparent places of Moon and other object, M and S their true places;



then $m\ s$ is apparent distance, M S the true. With pole Q describe small circles S q and M p, then

$$qp = \text{MS, the true distance.}$$

Now if

d' be the apparent distance,
d „ true „
C „ correction for ☽'s altitude
c „ „ ☉ „

then

$$\begin{aligned} SM &= qp = sm - pm + sq \\ &= d' - C \cdot \cos\theta + c \cdot \cos\phi \\ &= d' - C\left(1 - 2\sin^2\frac{\theta}{2}\right) + c\left(1 - 2\sin^2\frac{\phi}{2}\right) \\ \therefore\ d &= d' - (C - c) + 2\left[C \cdot \sin^2\frac{\theta}{2} - c \cdot \sin^2\frac{\phi}{2}\right] \ldots \text{I.} \end{aligned}$$

Let

m = moon's apparent altitude. z = moon's apparent zenith distance.

s = other objects' „ „ z' = other objects' „ „ „

$$S' = \frac{z + z' + d'}{2} \quad \text{and} \quad S = \frac{m + s + d'}{2}$$

Now

$$\begin{aligned} \sin^2\frac{\theta}{2} &= \frac{\sin(S' - d') \cdot \sin(S' - z)}{\sin d' \cdot \sin z} \\ &= \frac{\sin\dfrac{z' + z - d'}{2} \cdot \sin\dfrac{z' + d' - z}{2}}{\sin d' \cdot \cos m} \\ &= \frac{\sin\left[90 - \dfrac{m + s + d'}{2}\right] \cdot \sin\dfrac{m + d' - s}{2}}{\sin d' \cdot \cos m} \\ &= \cos S \cdot \sin(S - s) \cdot \operatorname{cosec} d' \cdot \sec m. \end{aligned}$$

Similarly

$$\sin^2\frac{\phi}{2} = \cos S \cdot \sin(S - m) \operatorname{cosec} d' \cdot \sec s.$$

From I

$$\begin{aligned} d &= d' - (C - c) + 2\cos S \cdot \operatorname{cosec} d' \,[C \cdot \sin(S - s) \cdot \sec m - c \cdot \sin(S - m) \sec s] \\ &= d' - (C - c) + 2\cos S \cdot \operatorname{cosec} d' \,[M - N] \end{aligned}$$

if

$$M = C \cdot \sin(S - s) \sec m,$$

and

$$N = c \cdot \sin(S - m) \sec s.$$

Of course a similar formula could be obtained by taking

$$\cos\theta = 2\cos^2\frac{\theta}{2} - 1 \text{ and } \cos\phi = 2\cos^2\frac{\phi}{2} - 1 \text{ in 3rd line.}$$

EXAMPLE.—The apparent altitude of the Moon is 54° 17′ 46″, that of the Sun 63° 18′ 48″; the apparent central distance is 41° 17′ 14″. The Sun's correction for altitude is 25″, the Moon's correction 33′ 24″. Find true distance.

$m = 54\ 17\ 46$ sec ·233888

$s = 63\ 18\ 48$ ... ... sec ·347646

$d' = 41\ 17\ 14$ ... ... cosec ·180565

$2)\ 158\ 53\ 48$

$S = 79\ 26\ 54$ ... ... cos 9·262741

$S - s = 16\ 8\ 6$ sin 9·443891

$S - m = 25\ 9\ 8$ ... ... sin 9·628414

$C = 33'\ 24'' = 2004''$ log 3·301898

$c = 25$ ... ... log 1·397940

$M = 954\ 28$ log 2·979677

$N = 23·66$ log 1·374000

$M - N = 930·62$ log 2·968772

$C \cdot \sin\frac{\theta}{2} - c \cdot \sin\frac{\phi}{2} = 258·3''$ log 2·412078

$\frac{\theta}{2} = 516·6 = +8'\ 36''6$

$C - c = -32\ 59$

$-24\ 22·4$

$d' = 41\ 17\ 14$

True distance $d = 40\ 52\ 51·6$

Borda's method gives ... ... $d = 40\ 52\ 58$

Krafft's „ „ ... ... $d = 40\ 52\ 30$

This question is taken at random from a work on Nautical Astronomy.

Abnormal Appearance of Jupiter's Satellite IV. while in Transit on March 12. By Henry Pratt.

At 10.30 P.M. the satellite was seen as a conspicuous spot on the great white belt just south of the equatorial zone, and it was observed until 11^h with power of 270, maintaining the whole time the same appearance, which I think is unusual even among the many dark transits which have been observed. Its form was the most singular feature, for instead of being round it was much elongated (elliptical or gibbous), and its major axis was not quite perpendicular to *Jupiter's* equator, but slightly in-

clined to it. It may be well to mention that this distortion of form was not caused by maladjustment of my speculum, because I have an original apparatus for testing that, and for adjusting by when needed, and which reports most sensitively the slightest deviation. I knew, therefore, the cause was not instrumental.

The general tone of the satellite was very dark, but not nearly as black as a shadow, although in the absence of such a comparison it might have been supposed to be black by an inexperienced observer. Its preceding margin and northern end were marked by a really black shading, which had very much the form of an inverted comma. Had this black shading been of the nature of a dark limb, it should have appeared on the opposite side of the satellite.

18 *Preston Street, Brighton.*

Occultations of λ *Geminorum and* κ *Cancri.* By Henry Pratt.

Occultation of λ *Geminorum on March* 6.

It occurred about the predicted time, but I omitted to note time precisely, owing to an unexpected phenomenon. The disappearance was observed by a power of 270 focussed very sharply on the star. Definition fair, but at times slightly unsteady.

While preparing to note the time of disappearance a diminution of the star's brightness suddenly commenced, and automatically at same instant I commenced counting beats of the sidereal clock. When I had counted the third second the star quite suddenly disappeared completely, and during the interval, which I estimated $= 3^{s}{\cdot}2$, its brightness was *gradually diminishing*, until when last seen it was not more than one-fourth of the brightness it exhibited before the phenomenon began. I watched for half a minute, but no reappearance or projection on Moon's limb took place. The reappearance occurred within the opened webs of the micrometer, but was not seen until the star had emerged about 1^{s}, owing to much rippling of the Moon's limb which commenced a few minutes previously.

Occultation of κ *Cancri on March* 8.

The disappearance was observed through haze with power 270 focussed (as sharply as the state of definition would allow) upon the star. It occurred at $10^{h}\ 18^{m}\ 46^{s}$ G.M.T., and was what is

usually termed perfectly instantaneous. Certainly it did not occupy ·2 of a second, but I thought it took place by a gradual diminution of brightness, although the time was so very short. Reappearance unseen, because at that time clouds covered the Moon.

18 *Preston Street, Brighton.*

The Radiant Points of Fireballs. By W. F. Denning.

There are certain dates of the year when fireballs have displayed a marked ascendency, and when their radiant points have become determinable from the path directions of such as have been accurately recorded. The catalogues of various meteor observers comprise a large number of these observations, though the relative proportion of meteors equalling or exceeding *Jupiter* is somewhat less than three per cent. of the aggregate number registered. In addition to this source of information there are the Annual Reports of the British Association Committee on Luminous Meteors, in which are published a large number of important computations (chiefly by Prof. A. S. Herschel) in connection with the heights, velocities, and radiant points of fireballs. Combining these results with my own and other observations of meteors during recent years, I have endeavoured to find the positions of some of the leading fireball radiants of the year.

In an investigation of this kind many difficulties have to be confronted, for a considerable number of instances are met with where the evidence of certain showers cannot yet be regarded as proved with sufficient distinctness. It is also doubtful, owing in many cases to insufficient materials, as to which are the systems entitled to precedence. There is also another point of signal importance with respect to the *durations* of these displays of large meteors. At first I attempted to group the radiants into very short periods of about two or three nights, but found the observations incompatible with such limited duration. In some well-defined examples they appear to extend over several weeks, and in fact present an analogy to showers of ordinary shooting stars, with which they are no doubt intimately associated; and as further records accumulate, the probable identity of such streams will be yet more closely established. As evidence of the long duration referred to, I may mention the shower in Scorpio (June–July) and Gemini (Nov.–Dec.), which obviously continue in action during a lengthy interval, as the following instances of doubly-observed fireballs (computed by Profs. Niessl and Herschel respectively) will sufficiently prove:—

Date.	Radiant. α	δ	Date.	Radiant. α	δ
1883 June 3 ...	249°9	−20°2	1862 Nov. 27 ...	102	+26
1883 3 ...	248	−20	1877 Dec. 9 ...	112	+27
1878 7 ...	249	−21	1863 12 ...	105	+27
1873 17 ...	248·6	−20·2	1873 24 ...	115	+38
1879 July 13 ...	246	−19			

The latter is also very rich in October, at the epoch of the *Orionids*, when it furnishes some very swift and, occasionally very brilliant, meteors in the morning sky :—

Radiant Points of Fireballs.

No.	Epoch.	Radiant. α	δ	Approximate Star.
I.	Jan. 1–3	230°	+51°	β Boötis
II.	4–12	57	−12	γ Eridani
III.	Mar. 1–4	300	+80	κ Cephei
IV.	30–Apr. 7	27	+18	α Arietis
V.	Apr. 11–12	106	+46	16 Lyncis
VI.	19–20	274	+37	α Lyræ
VII.	19–23	258	+37	τ Herculis
VIII.	19–23	210	− 9	κ Virginis
IX.	29–May 5	335	− 9	θ Aquarii
X.	June 3 7, 17	247	−25	α Scorpii
XI.	July 25–30	326	− 9	β Aquarii
XII.	25–30	8	+34	β Andromedæ
XIII.	25–31	338	−11	δ Aquarii
XIV.	25–31	303	+12	α Aquilæ
XV.	25–Aug. 1	32	+53	χ Persei
XVI.	Aug. 9–11	4	+56	η Persei
XVII.	9–11	12	+30	β Andromedæ
XVIII.	9–11	335	+73	β Cephei
XIX.	9–11	284	+62	o Draconis
XX.	9–11	312	+50	α Cygni
XXI.	19–22	330	+69	β Cephei
XXII.	19–25	291	+60	δ Draconis
XXIII.	Sept. 1–4	306	+54	α Cygni
XXIV.	6–15	61	+36	ε Persei
XXV.	10–25	336	+35	π Pegasi
XXVI.	Oct. 2	225	+52	β Boötis
XXVII.	15–22	92	+15	ξ Orionis

No.	Epoch.	Radiant. α	δ	Approximate Star.
XXVIII.	15–22	106°	+23°	ϵ Geminorum
XXIX.	17–22	40	+20	ϵ Arietis
XXX.	17 26	247	+41	η Herculis
XXXI.	25–Nov. 7	48	+22	ϵ Arietis
XXXII.	Nov. 5–8	58	+16	γ Tauri
XXXIII.	12–14	148	+23	γ Leonis
XXXIV.	19–23	62	+22	ϵ Tauri
XXXV.	27	29	+46	γ Andromedæ
XXXVI.	Dec. 6–8	80	+23	ζ Tauri
XXXVII.	6–12	106	+32	α Geminorum.

Several of the above showers ought perhaps to have been coupled together as identical. XV. is certainly an early display of the Perseids (XVI.). The Andromedes (XII. and XVII.), Cepheids (XVIII. and XXI.), Draconids (XIX. and XXII.), Cygnids (XX. and XXIII.), Muscids (XXIX. and XXXI.), and Taurids (XXXII. and XXXIV.) are so nearly conformable to the same respective positions that it is impossible to resist the inference that these pairs are closely allied, if not actually connected, with each other.

I have solely relied on my own observations and reductions for the positions given in the table. In cases where the same radiant has been determined by other observers the agreements are usually very satisfactory, though not, in two or three instances, as close as might be expected. Thus No. IX. is Col. Tupman's shower of May 2, 1870, which he placed at 325°−2½°, so that there is a difference of about 11° between the two results. The June shower in *Scorpio*, which seems a very important one, is given by Prof. Niessl at 248°−20,* but I judged it to lie at 247°−25° from the large fireball which I, in common with many others, observed on June 7, 1878, 9h 50m. The July Aquariads (Tupman, No. 43), at 341°−13°, which returned with great activity in 1878 and few following years, are doubtless identical with the fireball radiant No. XIII., which has been derived from a variety of sources. I have not included in the list such radiants as have been ascertained by duplicate observations of isolated fireballs, of which there are probably some hundreds. It will be found, however, that many of the showers now given receive close confirmation from such observations. As to the showers in *Perseus* (No. XXIV.), in *Musca* (No. XXXI.), and near ζ *Tauri* (No. XXXVI.) they seem to have escaped special

* *Astronomische Nachrichten*, No. 2566. Prof. Niessl's computation of the position of this radiant is probably very exact as the several observations of large fireballs which he has discussed show an excellent corroboration.

notice until the last few years, although they severally furnish displays of considerable richness.

In addition to the centres included in the summary, there are other showers which unquestionably deserve a place, though it seems necessary not to deviate from the original plan of giving none but those of the most prominent character. One or two instances may, however, be adduced as representatives of a numerous class:—

January 12–19, from *Cancer*.—The large fireball of Jan. 12, 1879, had a radiant at 133°+19°. Another fireball, seen on Jan. 19, 1877, was directed from 135°+27°, and probably emanated from the same system.

February–March.—Some large fireballs from *Sextans* (145°+6°).

March.—Fine meteors diverge from S. of *β Leonis* (175°+14°).

May 30.—A fireball radiant in *Cassiopeia* (18°+58°).

June–July.—Several fireballs from the northern limits of *Sagittarius* (285°−12°).

Such references to showers more or less certainly established from past observations might be greatly extended through each of the last six months of the year, until on Dec. 30 we come to a display from the extreme north-east region of *Auriga* (92°+57°), to which the great meteor of Dec. 30, 1878, owed its origin.

There is also a notable fireball epoch on Feb. 7–10 and Dec. 21, but I have failed to discover the places of radiation from a sufficient number of observations.

I had intended to annotate the positions included in the table, with remarks explanatory of their probable strength and the sources of corroboration, but the addition of such matter must have extended the paper to considerable length beyond its purpose. It will be noticed that all the well-known periodical showers are represented in the list. I found it necessary to include them, as they have each, at their several epochs, supplied many fine meteors, though neither the Orionids or Andromedes are remarkable for their fireballs. The special nights of recurrence of all the chief periodical showers are, however, those upon which very extended watches have been kept, and when the rich showers alluded to have asserted their supremacy over the more attenuated contemporary displays not only as regards shooting stars of normal aspect, but (from their much greater intensity) as regards large meteors also.

At some epochs prolific in fireballs, such as those of March 1–4 and April 11–12, it has been found difficult to decide as to the principal radiant point, and the positions adopted in these cases must be understood as provisional, and subject to such alteration as future observations may render necessary. Early in March there are several large meteor showers proceeding from *Leo*, *Ursa*, and *Camelopardus*; and about April 11–12 the observations clearly indicate the positions of three radiants,

in addition to that given in the list, at 70°+50°, 159°−8°, and 18°+57°. In these and other similar cases the want of additional data is greatly felt; not only is it required in support of showers tolerably well known, but of others whose existence our scanty records have given us only the mere suspicion. Amongst the latter there are probably many important systems, and hence the desirableness of further pursuing these investigations. The present catalogue is preliminary to what it is hoped future observations will enable to be greatly extended and revised. The necessity for closer attention to this interesting branch of astronomy is admitted, and its great significance has long since been recognised.

I believe these fireball radiants are equally numerous with the star showers of ordinary character, and are often identical as regards physical origin. With reference to the Taurids of November I have observed several instances of minute telescopic meteors and of the largest type of fireballs belonging to this shower. The epoch and place of the radiant agree so closely that, notwithstanding the enormous difference of magnitude in the phenomena observed, there can be no question that they are derived from the same original source. With respect to the number of the positions comprised in the list, they might have been greatly augmented by the addition of many streams which have manifested themselves with more or less certainty in recent years. But it seemed desirable to omit these, awaiting further confirmation, and thus avoiding, as far as possible, the risk of pseudo-radiants, or of radiants of such markedly feeble character as rarely to give any visible indication of their presence.

In observations of fireballs there is a great need of uniform and accurate description. Amongst observers so widely different as regards astronomical knowledge, and so variously impressed as to the essential features of such records, we cannot wonder that their results are often very discordant, and sometimes of no practical utility whatever. The observer is generally taken unawares, and seldom manages to obtain good positions of a fireball's track (even if he be practised in such work), unless by means of the enduring streak, which so frequently remains as an unfailing indication of it. But this can only be considered as such in the first few seconds of its projection, before it will have commenced to drift and assume that contorted appearance which is so common a feature of these meteoric afterglows. I have sometimes noticed a considerable drift in fireball streaks, and the grotesque forms they occasionally put on before ultimate extinction are very striking. Observers should, therefore, note the path from the direction of the streak in its primitive stage, and prolong the extremities (in estimating the whole length of the arc traversed by the nucleus) a sufficient extent on marking the course upon a chart or globe. The greatest intensity of the streak invariably occurs at the point of maximum outburst, near

the end of the visible flight of the meteor, and the streak is often divided into sections or much condensed in certain parts, according to the remarkable fluctuations which frequently occur in the brightness of the nucleus as its derivative source. The point of greatest brilliancy of a meteor will, therefore, be found to conform with the brightest region of the streak. It is always best to give the path in R. A. and Decl. rather than attempt description by certain neighbouring stars, the latter method being less accurate and more troublesome in reference. Nor should the meteor be described by altitudes or compass bearings, unless in cases where correctness is ensured by instrumental comparisons, or where the apparition occurs in daylight or hazy weather, when it is not feasible to otherwise assign places. Ordinary estimations of altitude are enormously in excess, and it is often doubtful, with regard to given compass-bearings, whether they are true or magnetic. Whenever the observer is not sufficiently versed in the constellations to assign the path in any other manner than by random guesses at the altitude and azimuth, it is best to reject such observations, unless by combination with many other results, which may serve as a corrective. By comparing several observations, in which the alt-azimuthal points of a fireball have been carefully measured, they will doubtless exhibit an excellent consistency, enabling the radiant point and other elements to be ascertained with precision; but in general it must be admitted that a uniform method of recording meteor flights by degrees of R. A. and Decl. + or −, is preferable to any other, as avoiding the confusion and troublesome references involved in different modes of description.

The systematic watching for very large meteors, when not undertaken in conjunction with that of ordinary shooting stars, is likely to exhaust the patience of the most enthusiastic observer, and is therefore not to be recommended. Meteoric observations are best followed on the approved plan, which admits the necessity of noting the faintest objects visible. And these observations should be sustained irrespective of special epochs and the paths and durations of meteors registered in a convenient form. What would accelerate the development of our knowledge of fireballs more than perhaps any other circumstance would be the *general* attention of astronomical and meteorological observers, could we command it, to the accurate description of the more salient characteristics of these phenomena. Were such a plan followed a few years would obtain for us an enormous increase in our records, and enable many doubtful points to be cleared up. Observers would do well to practise themselves in this department of research, and to contribute more freely to its slowly accumulating facts. Co-operation in a sphere of this character where the individual objects to be observed are incapable of definite prediction either as regards time or place of appearances is very essential, for even in the

habitual observation of fireballs a single observer will gather comparatively few materials unless his labours are prolonged over a very long series of years. The work to be thoroughly successful must be undertaken at many different stations. This has, indeed, to some small extent, been already effected, and as the result we have the valuable meteor catalogues of Heis, Schmidt, Zezioli, Weiss, Konkoly, Schiaparelli, Denza, and others. But notwithstanding this diligence at a few isolated observatories, our existing materials are, it must be confessed, deficient in the extreme, and quite inadequate to the purpose of a satisfactory discussion. Such systems of fireballs as we have already been enabled to detect by means of the few scattered data in our possession may be fairly assumed to constitute streams of great importance, and to afford some notable examples of the multitude of such showers which are displayed with more or less distinctness every year.

Bristol, 1884 *March* 15.

Some Remarks on the Chain of Meridian Distances, measured around the Earth by H.M.S. "Beagle" between the years 1831 *and* 1836. By Prof. A. Auwers.

A great difference between some recent determinations of longitudes in the Straits of Magellan and the results, apparently well founded, and regarded as fundamental up to the present day, in the geography and hydrography of South America, obtained by H.M.S. "Beagle" half a century ago, has been the reason of my undertaking some fresh computations concerning the chronometrical measurements of differences of longitudes which formed one of the main objects of the celebrated second cruise of the said vessel, under the command of Capt. R. Fitz-Roy, during the years 1831 to 1836.

Fitz-Roy has given * as the definitive result for the longitude of the "Old Observatory" at Port Famine, which formerly served as the fundamental point for the surveys of Tierra del Fuego, $4^h\ 43^m\ 51^s{\cdot}5$ W. But if we take from Fitz-Roy's results only the difference measured between Monte Video and Port Famine—for which he assumes as the value mostly to be relied upon $58^m\ 58^s{\cdot}68$ according to the ten chronometers most nearly agreeing in the two runs constituting the connection, from Monte Video to Port Desire, and from Port Desire to Port Famine; which value, however, would be diminished by $2^s{\cdot}19$, using the mean of all the 17 and 16 chronometers respectively—and if we substitute for his own result for Monte Video the longitude as determined recently

* *Narrative of the Surveying Voyages of His Majesty's Ships "Adventure" and "Beagle,"* Appendix to vol. ii. p. 348.

with much greater precision by telegraph,* this value ought to be increased to $4^h\ 43^m\ 55^s{\cdot}11$. This result, which would appear very trustworthy from the excellent harmony between the separate results of the great majority of the chronometers made use of, furthermore seems to receive a complete and strong corroboration from that derived by Capt. King from the operations of the preceding surveying expedition to Tierra del Fuego;† on the contrary, from the results of the German Transit of *Venus* Expedition 1882 IV., the true value appears to be nearly $4^h\ 43^m\ 45^s$, this number being subject to an uncertainty, amounting perhaps to 2^s, from the circumstance only that there remain some doubts concerning the connection between the German Observatory at Punta Arenas and the old observation spot at Port Famine, distant about thirty nautical miles.‡

There was, therefore, a discordance amounting to 10^s, which it seemed impossible to explain by accidental errors of the measurements, and which induced me to examine whether the old determination might be affected by systematic errors, and if possible to eliminate such errors. Before reporting upon this investigation, it seems necessary to refer to some details of the arrangement and the execution of the "Beagle's" measurements.

For her second voyage, begun at the end of the year 1831, the "Beagle" was equipped with twenty-two chronometers, which were placed on board and managed during the voyage with the utmost care, limits being imposed on the latter only by the necessity, inevitably connected with the circumstances of the undertaking, of having the chronometers kept going through a period of six years without being cleaned, and for a number of them by the necessary use in surveying operations.

Unfortunately no material of any kind has been given wherefrom to form a judgment of the absolute and relative accuracy of the different chronometers, all of which, of course, when delivered to the "Beagle," will have been considered to be superior instruments, except the separate values given by each chronometer for the several meridian distances, and a kind of classification made by Fitz-Roy, probably founded upon his general impression from the performance of the single chronometers during the voyage. The following list contains the statement of the class of instruments he considered them to be. Next to this I give the relative weights, differing materially in some instances from Fitz-Roy's classification, which I have been ultimately led to assume:—

* *Telegraphic Determination of Longitudes on the East Coast of South America in* 1878 *and* 1879. Washington, 1880.

† *Narrative*, vol. ii. App. p. 322; difference with Monte Video, $58^m\ 58^s{\cdot}07$, being the mean of 54 separate chronometrical results, which considered singly might appear less accurate than those of Fitz-Roy, but in the mean result will make this difference perhaps even more than good by reason of their being gathered in several, probably six or eight, voyages made in both directions.

‡ See Note following this paper.

Box Chronometers.

Letter.	Maker and No.	Fitz-Roy's remarks.	Weight.
A	Molyneux 1415	good	4
B	Gardner 24	bad	$\frac{1}{4}$
C	Molyneux 1081	rather good	2
D	Murray 542	,,	1
E	Eiffe E	,,	$\frac{1}{2}$
F	Arnold and Dent 661	,,	$\frac{1}{2}$
G	,, ,, 633	,,	1
L	Arnold 634	,,	$\frac{3}{4}$
M	Frodsham 2	,,	$\frac{1}{2}$
N	Molyneux 1175	,,	$1\frac{1}{2}$
O	Earnshaw 705	,,	3
P	Frodsham 1	bad	$\frac{1}{2}$*
R	Murray 584	very good	3
S	Arnold 465	rather good	2†
W	Molyneux 971	good	$1\frac{1}{2}$
X	Earnshaw 509	rather good	$\frac{1}{2}$
Z	French 4214	good	4

Pocket Chronometers.

H	Arnold and Dent 261	rather good	$\frac{1}{2}$
K	Parkinson and Frodsham 1042	good	$\frac{1}{4}$‡
T	Molyneux 1326	indifferent	0
V	Pennington 426	rather good	$\frac{1}{2}$
Y	Morrice 6144	,,	$\frac{1}{8}$

With a view of easily distinguishing the pocket chronometers from the others, in what follows their letters are put in parentheses.

With these chronometers—strictly speaking with a number of them gradually diminishing during the course of the voyage—twenty-seven fundamental differences of meridians have been measured; the two first and the five last constituting a double connection between Bahia and England, and the remainder forming the twenty links of the celebrated "chain of longitudes" extending round the earth, proceeding westward from Bahia and returning to the same place. The first of these links, Bahia—Rio de Janeiro, was measured three times; because a large discordance between the result of the first measure-

* Appeared entitled to weight 1 but for Fitz-Roy's remark.
† In Section III. (see below); in the earlier parts of the voyage $p=\frac{1}{8}$.
‡ Employed regularly in the observations.

ment with another made shortly before by Baron Roussin, immediately perceived by Fitz-Roy after the arrival at Rio, made an immediate control desirable, which was effected by sailing back at once for Bahia. All the other links of the chain depend on single measurements. The local times to be compared were always determined by equal altitudes of the sun taken with a very good sextant; the rates were determined by observations of the same kind, the stoppages at the different stations, in most instances, being long enough to allow the rates to be derived from a comparison of the first and last sets of equal altitudes with an accuracy fully adequate to the presumable accuracy of the transport of local time from one station to another; only towards the end of the voyage the stoppages sometimes appear rather too short. To carry the local time from one meridian to another, the two including determinations of rate have been made use of; but it is not stated whether the means of the two rates have been used, or the change applied exactly proportional to the time.* In the great majority of cases, however, both modes of computing would have led to very nearly the same results.

The only particulars of the measurements published in Fitz-Roy's Report† are: the spots where the observations for time were made at the different stations; the differences found, as the results of a computation remaining unknown as to all its details, between each two consecutive meridians by the single chronometers—wherein it is somewhat striking that in no single instance results of all the twenty-two chronometers are given; and the interval between the last observation for time at the preceding and the first at the following place. By means of the data contained in the "Abstract of the Meteorological Journal,"‡ and from occasional notices given in the narrative, the dates of these observations themselves can be made out within one or two days in most instances; and in every instance the exact times of the runs from one place to another which ought to serve as an essential element in the following investigation. From the Meteorological Abstract can be derived at least some continuous indications of the temperature of the chronometers during the several runs and ratings, as their temperature can be supposed, without fear of much error, except some few periods, to have nearly agreed with the temperature of the cabin barometer; but there remains the serious drawback, that the mean daily temperatures cannot be more than approximately estimated, on account of there being generally only one reading of the attached thermometer—and only one of the observations for the temperature of the air—given for each day; most of these observations

* An express statement concerning the method adopted to allow for the rates, vol. ii. App., p. 330, appears to relate not to the determination of the fundamental differences, but to the insertion of secondary meridians.

† *Narrative*, vol. ii. App., pp. 331–344.

‡ *Ib.*, vol. ii. App., pp. 1–62.

are taken during the warmer half of the day, but there is a rather large difference between the regular hours of observation in different periods of the cruise.

The following enumeration contains:—a summary of the measured differences of meridians and the designations by which they are referred to in the subsequent investigation; the (approximate) dates of the observations for time next including each single run; the actual duration of the run; and the approximate mean temperatures in which the chronometers were rated before and after the run, and in which they were kept between the two periods. The numbers given as such temperatures are the simple means of all readings of the attached thermometer given for the interval in question in the Meteorological Abstract for the hour of observation, regular for that period of the voyage; strictly speaking, therefore, the adopted numbers represent temperatures, which generally—except perhaps the first and the last measurement—exceed the actual mean temperature of the chronometers during the interval in a slight but slowly varying degree.

Besides these data, I give in the following summary other values of the longitudes of Fitz-Roy's stations in those instances where more certain determinations than those of Fitz-Roy himself—or equally certain ones in some few instances—exist, or have come within my knowledge.

a. Devonport to Porto Praya (S. Jago, Cape Verde Islands). Observations, 1831 Dec. 27, 1832 Jan. 16. Duration of the run, 20 days. Mean of observed temperatures: at Devonport, 52° (uncertain); during the run, 64°; at Porto Praya, 76°.

Place of observation at Devonport: The Baths, in the meridian of Government House, 1′ 48″·1 W. of Plymouth old church. The table of positions in the *Connaissance des Temps*, the only source wherefrom I can derive the longitude of this starting point, states Plymouth new church to be 6° 28′ 14″ W. of Paris,* value which appears to have originally been 4° 8′ 5″ W. of Greenwich. The old church being 14″·9 W. of the new church,† the longitude from which to reckon the "Beagle's" measurements is 4° 10′ 8″ = $16^m\ 40^s{\cdot}53$ W.

Place of observation at Porto Praya: landing place at the west side of Quail Island.

b. Porto Praya to Bahia. Observations 1832, Feb. 5, March 2. Run, 19 days. Temperatures, 76°, 80°, 82°.

Place of observation at Bahia: Fort S. Pedro. By the U. S. N. Expedition the longitude of the lighthouse at S. Antonio has been determined, in 1879, $=2^h\ 34^m\ 8^s{\cdot}37$. The relative situation of the two spots I can only take from a small

* This number depends on the Trigonometrical Survey of England and Wales, multiplying the meridional distances as computed by Mudge by the constant factor 1·003293.

† *Narrative*, vol. ii. App. p. 321.

(1 : 150000) but carefully drawn plan, which makes Fitz-Roy's station $2^{s}\cdot95$ E. and $1'\ 30''\cdot6$ N. of S. Antonio.

1. Bahia—Rio de Janeiro. First measurement, 1832 March 15–April 6. Run, 17 days. Temperatures, 82°, 82°, 76°. Second measurement, in opposite direction, May 10–16. Run, 5·5 days. Temperatures, 76°, 76°, 80°. Third measurement, May 22–June 4. Run, 11 days. Temperatures, 80°, 76°, 74°.

Place of observation at Rio: Fort Villegagnon. Longitude determined by the U. S. N. Expedition $=2^{h}\ 52^{m}\ 37^{s}\cdot95$.

2. Rio de Janeiro to Monte Video. Observations, 1832 July 4, July 27. Run, 21 days. Temperatures, 74°, 66°, 58°.

Place of observation at Monte Video: Rat Island, according to King,* $1'\ 55''$ west of the Cathedral; according to Fleuriais,† $7^{s}\cdot16$. Taking the mean of these two values, from the U. S. N. Expedition's longitude of the S.E. tower of the Cathedral $=$ $3^{h}\ 44^{m}\ 49^{s}\cdot02$, the longitude of the spot so frequently employed by English vessels to rate their chronometers is: $3^{h}\ 44^{m}\ 56^{s}\cdot43$.

3. Monte Video to Port Desire (Spanish ruins). Observations, 1833 Dec. 5, Dec. 24. Run, 17 days. Temperatures, 69°:, 62°, 61°:.

4. Port Desire to Port Famine (old Observatory, at the west side of the port). Observations, 1834 Jan. 21, Feb. 5. Run, 11 days. Temperatures, 61°:, 59°, 54°. For the longitude of Port Famine see the note appended to this paper.

5. Port Famine (new Observatory, $5^{s}\cdot9$ E. of former place) to Ancud (S. Carlos, Chiloë, observation spot: Punta Arenas). Observations, 1834 June 7, July 3. Run, 19·5 days. Temperatures, 43°, 49°, 50°.

6. Ancud to Valparaiso. Observations, 1834 July 13, July 25. Run, 8 days. Temperatures, 50°, 55°, 56°.

Place of observation at Valparaiso: Fort S. Antonio, in the same meridian with the permanent point to which M. Fleuriais has referred (1868) his determination of the Cerro de la Cordillera. From some slight modifications of the results given by Gilliss and Moesta and a new reduction of the French observations‡ I find the most probable value of the longitude, giving equal weight to the four available determinations, $4^{h}\ 46^{m}\ 30^{s}\cdot84$.

For the difference between Ancud and Valparaiso the "Beagle's" voyage would afford another determination from the direct backward run in November 1834, which would appear to deserve equal confidence, but is not given.

7. Valparaiso to Callao. Observations, 1835 June 27, July 21. Run under sail, 15 days, rather long stoppages intervening at Coquimbo and Iquique. Temperatures, 58°, 63°, 65°.

Place of observation at Callao: the Arsenal. To the same meridian, within the limits of the probable errors in the observations for time, the telegraphic connection with M. Fleuriais'

* *Narrative*, vol. i. p. 479. † *Conn. d. T.* 1870, Addit. p. 47.
‡ *Astron. Nachrichten*, No. 2587.

station Pisco, effected by Lieuts. Leclerc and Benazé,* appears to refer. Adding the difference as found by these officers $=3^m\ 43^s{\cdot}75$ to my result from M. Fleuriais' observations,† the longitude becomes $5^h\ 8^m\ 37^s{\cdot}17$.

8. Callao to Galapagos Islands, Chatham Island (landing place at the S.W. side of Stephen's Bay). Observations, 1835 Sept. 6, Sept. 18. Run, 9 days. Temperatures, 65°, 69°, 72°.

9. Galapagos Islands, Chatham Island, to Charles Island (landing place at the S.E. part of Post Office Bay). Run, 2 days. Temperature, 72°.

10. Charles Island to Otaheite, Point Venus. Observations, 1835 Oct. 16, Nov. 17. Run, 29·5 days. Temperatures, 72°, 77°, 80°.

The longitude of Point Venus is, according to Dr. Powalky's computation of two occultations observed by Cook 1773 and 1774, and the only available one of the two occultations observed 1824 by Preuss $= 9^h\ 57^m\ 56^s{\cdot}1$ W.‡ A determination of the longitude of Motu Uta, in the harbour of Papeete, made in 1859 by officers of the *Novara*,§ affords the following results:

	h	m	s
Longitude from 2 culminations of the Moon's first limb ...	9	58	11·29
„ 3 „ „ second limb ...	9	58	19·22
Mean	9	58	15·25
Longitude from the occultation of *Regulus* (disapp.), 1859 Feb. 16	9	58	11·7

On the same small island was situated the Observatory of M. A. Kulczycki, the longitude of which has been found, "by long series of lunar culminations," 151° 54′ 30″ W. of Paris,‖ which should correspond to $9^h\ 58^m\ 17^s{\cdot}4$ W. of Greenwich. The observations of M. Kulczycki not being published, it is not possible to correct exactly some systematic error, which might be attributed to their computation as well as to the Paris reduction of M. Fleuriais' observations made about the same time; we might arrive at this correction approximately by assuming the difference of the equinoxes, to which the places of the Moon and of the stars have been referred in the computation, equal to its mean value for all M. Fleuriais' series $= 0^s{\cdot}025$, and multiplying this difference by a mean factor 26, whence the correction to the computed longitude becomes $+0^s{\cdot}65$.

M. Kulczycki's Observatory was situated $20^s{\cdot}00$ west of Point

* *Conn. d. T.* 1873, addit. p. 21. † *Astr. Nachr.*, as before.

‡ *Astr. Nachr.* No. 1459.

§ *Reise der Oesterreichischen Fregatte Novara um die Erde. Nautisch-physikalischer Theil*, p. 29. The result of the occultation is there given as $9^h\ 58^m\ 13^s{\cdot}3$. This number ought to be diminished by (approximately) $1^s{\cdot}6$ from a more correct interpretation of the Greenwich results for the errors of the tables of the Moon.

‖ *Conn. d. T.* 1873, Add. p. 4.

Venus. The Austrian determination may be transferred to this point without risk of sensible error with the same value of the difference; then the three results and their mean will be:—

	h	m	s
Occultations of Cook and Preuss	9	57	56·1
Novara	9	57	53·5
M. Kulczycki	9	57	58·0
Mean	9	57	55·9

11. Otaheite to Paihia (Bay of Islands, New Zealand). Observations, 1835 Nov. 25, Dec. 23. Run, 25 days. Temperatures, 80°, 75°, 69°.

12. Paihia to Sydney. Observations, 1835 Dec. 29, 1836 Jan. 17. Run, 14 days. Temperatures, 69°, 71°, 73°.

Place of observation at Sydney: Fort Macquarie. By transporting three chronometers to the Paramatta Observatory and back, Fitz-Roy found his place $52^s{\cdot}0$ east of the Observatory. The longitudes ultimately given for the latter (by Rümker* $10^h\ 4^m\ 6^s{\cdot}25$, by Wurm† $10^h\ 4^m\ 4^s{\cdot}1$) remain rather largely in error; but a pretty accurate determination can be derived from the position of the present Sydney Observatory, situated $2^s{\cdot}59$ west of Fort Macquarie. The most reliable determination of the longitude of this Observatory, up to the present time,‡ I consider $10^h\ 4^m\ 49^s{\cdot}9$ E., which I derived some years ago from all published lunar culminations observed at Sydney and Williamstown, numerous occultations observed at Windsor, and the longitudes determined by the German and English Expeditions for observing the Transit of *Venus* 1874 (Auckland Islands and New Zealand).

13. Sydney to Hobart Town. Observations, 1836 Jan. 29, Feb. 10. Run, 6 days. Temperatures, 73°, 67°, 67°.

Place of observation at Hobart Town: "East side of Sullivan Cove, in a small battery close to the water"—probably Prince of Wales Battery, next to the S.E. point of the bay, which opens towards E. Recently the longitude of Hobart Town has been determined by telegraph from Melbourne, but I do not know of a published result.

14. Hobart Town to King George Sound (Government buildings, at the east side of Princess Royal Harbour). Observations, 1836 Feb. 16, March 7. Run, 18 days. Temperatures, 67°, 64°, 68°.

15. King George Sound to Keeling Islands (N.W. part of

* *Phil. Trans.* 1829, part iii. p. 29. The difference, evidently between the same two spots, is there stated to have been measured by Rümker, by frequent transports of chronometers, $= 51^s{\cdot}93$.

† *Astr. Nachr.* No. 200.

‡ Only just when sending this paper to the press, I received the February number of the *Monthly Notices*, wherein the result of the new telegraphic determination is mentioned as being $10^h\ 4^m\ 48^s{\cdot}93$. From Mr. Ellery's last Annual Report, however, even this result appears to be still open to correction.

Direction Islet). Observations, 1836 March 13, April 1. Run, 19 days. Temperatures, 68°, 74°, 82°.

16. Keeling Islands to the Mauritius. Observations, 1836 April 12, April 30. Run, 17 days. Temperatures, 82°, 81°, 79°.

Place of observation at Mauritius: Port Louis, battery on Cooper's Island (Fort George). An accurate value of the longitude is expected to result from Mr. Gill's chronometrical connections with Aden, 1874–5, which are not yet published.

17. Mauritius to Simon's Bay. Observations, 1836 May 9, June 2. Run, 22 days. Temperatures, 79°, 73°, 64°.

The place of observation (S.E. end of the dockyard at Simon's Bay), was found to be $10^s{\cdot}9$ west of the Royal Observatory near Cape Town by three chronometers. The longitude of the Observatory is, according to Mr. Gill, $1^h\ 13^m\ 54^s{\cdot}74$ E.*

18. Simon's Bay to St. Helena. Observations, 1836 June 19, July 8. Run, 19 days. Temperatures, 64°, 67°·5, 71°.

The observations were made at St. Helena in the meridian of the then existing Observatory, the longitude of which Johnson had found from a comparison of his lunar culminations with those observed at Greenwich, Cambridge, and the Cape, $22^m\ 54^s{\cdot}6$ E.† This determination, however, which Johnson himself considered to give a rather too great E. longitude, might be improved by adding to the comparisons computed by Johnson those with Abo, Munich, Edinburgh, Makerstoun, Cracow, and Prague computed by Daussy in the *Conn. d. T.* for 1837, and by substituting corrected values for the longitudes of the places of comparison. Deriving only a single result from each observation made at St. Helena, with a proper weight according to the number of observations compared, I find, first using only the observations of those places which afford comparisons of both limbs, viz. Greenwich, Cambridge, Cape of Good Hope, Abo, and Munich :—

	m s
Long. from 35 St. Helena observations of the first limb	=22 53·29 (weight=19·67 obs.)
Long. from 5 St. Helena observations of the second limb	=22 53·09 (weight= 2·83 obs.)
Mean	=22 53·19 (weight= 9·91 obs.)

excluding all comparisons differing more than 15^s from these means. In a second computation, including the comparisons with Edinburgh, Makerstoun, Cracow, and Prague, which occur in the first two years (1830–1), but are confined to the first limb, the result from this limb becomes: longitude from 39 St. Helena observations of the first limb = $22^m\ 52^s{\cdot}18$ (weight = 24·6 obs.), and the mean from both limbs = $22^m\ 52^s{\cdot}63$ (weight = 10·16).

The longitude of St. Helena would become more accurately known from Johnson's observations by comparing them with

* *Monthly Notices*, vol. xliii. p. 418.

† Johnson, *A Catalogue of 606 Principal Fixed Stars*, Introd. p. 32.

places of the Moon computed from Hansen's tables, because there remains a number of observations, and among these nine more observations of the second limb, for which no observations are available for comparison. For the present I take as the most probable result of Johnson's observations the mean of the above values $= 22^m\,52^s{\cdot}91$. The *Conn. d. T.* has much diminished in later editions (since 1869) the longitude of St. Helena, and gives now the value $22^m\,50^s{\cdot}9$, without referring to the authority for the new determination.

19. St. Helena to Ascension. Observations, 1836 July 14, July 20. Run, 6 days. Temperatures, 71°, 73°, 76°.

Place of observation at Ascension, centre of Barrack Square. Three occultations observed by Mr. Gill * at Mars Bay in 1877 would give for the same place, applying the difference $2^s{\cdot}07$ according to the Admiralty chart, $0^h\,57^m\,41^s{\cdot}45$ W., but this number is subject to a doubt of some seconds owing to the uncertainty of the positions of the Moon employed in the computation.

20. Ascension to Bahia. Observations, 1836 July 24, Aug. 2. Run, 9 days. Temperatures, 76°, 77°, 78°.

v. Bahia to Pernambuco. Observations, 1836 Aug. 6, Aug. 13. Run, 6·5 days. Temperatures, all equal, 78°.

The observation spot at Pernambuco, at the S.W. end of the Arsenal, appears to have been very nearly the same as that occupied by the American transit in 1878, in longitude $2^h\,19^m\,28^s{\cdot}96$ W.

w. Pernambuco to Porto Praya. Observations, 1836 Aug. 17, Aug. 31. Run, 13·5 days. Temperatures, 78°, 81°, 81°.

x. Porto Praya to Angra, Terceira. Observations, 1836 Sept. 5, Sept. 20. Run, 14·5 days. Temperatures, 81°, 78°, 75°. The observations were made at Angra, "close to the best landing place."

y. Angra to Devonport. Observations, 1836 Sept. 23, Oct. 7. Run, 9 days. Temperatures, 75°, 66°, 59°.

During this run the vessel stopped at Falmouth three days, observations for time being made at Pendennis Castle (longitude by *Conn. d. T.*, corrected from Trig. Survey, $20^m\,10^s{\cdot}93$ W.).

z. Devonport to Greenwich. Observations, Oct. 17, Oct. 27. Run, 10 days. Temperatures, 59°, 60°, and, very uncertain, 53°.

On the run a comparison of the chronometers was made at the R.N. College, Portsmouth (longitude from *Conn. d. T.*, as before,† $4^m\,24^s{\cdot}80$ W.).

The following table contains the differences of meridians as found by the single chronometers.

* *Mem. R.A.S.* vol. xlvi. p. 34.

† The *Nautical Almanac* appears still to retain the uncorrected longitude of the Trig. Survey, $4^m\,23^s{\cdot}9$.

TABLE I.

Meridian Distances.

Measured by Chron.	a	b	1^1	1^2	1^3	2	3	4	5	6
	$1^h\ 17^m$	$0^h\ 59^m$	$0^h\ 18^m$	$0^h\ 18^m$	$0^h\ 18^m$	$0^h\ 52^m$	$0^h\ 38^m$	$0^h\ 19^m$	$0^h\ 11^m$	$0^h\ 8^m$
	s	s	s	s	s	s	s	s	s	s
A	21·68	64·67	32·50	29·58	31·17	16·19	46·88	70·57	68·12	55·69
B	21·80	59·41	—	—	42·45	8·57	—	69·39	97·16	44·51
C	20·69	61·85	28·98	31·50	28·02	9·75	44·08	70·65	52·62	62·07
D	17·03	40·68	30·47	31·46	28·65	16·11	50·17	69·03	54·60	66·01
E	20·33	52·21	—	27·79	34·16	14·98	50·30	—	57·52	66·72
F	23·03	64·06	35·52	31·87	31·79	14·57	46·01	70·70	—	42·77
G	21·43	66·00	33·90	30·89	30·42	17·57	48·37	65·07	70·02	53·90
(H)	17·16	—	31·59	29·92	—	11·28	43·01	69·71	47·95	68·10
(K)	23·90	77·99	31·25	30·69	28·02	27·36	56·56	62·10	70·99	62·60
L	21·12	61·60	29·76	30·22	31·13	22·89	48·04	70·35	63·68	60·17
M	20·47	59·56	38·23	29·68	—	—	40·01	75·84	58·40	—
N	24·42	63·95	30·98	29·60	33·41	19·79	42·75	—	66·37	59·17
O	—	49·87	31·35	31·17	29·49	14·14	—	—	—	—
P	17·73	63·58	32·06	31·37	33·34	13·06	—	—	78·26	47·60
R	19·90	63·48	33·42	31·61	33·09	20·83	45·65	72·20	69·42	55·64
S	20·52	48·47	27·13	—	39·47	12·35	51·49	51·65	51·87	—
(T)	—	41·47	29·43	31·44	31·33	9·89	—	99·07	51·00	—
(V)	22·23	71·17	26·70	29·71	31·21	—	—	—	42·42	69·05
W	20·93	63·91	27·88	33·02	32·13	14·08	45·45	74·22	55·93	63·39
X	21·08	62·19	30·63	31·83	29·96	14·42	39·95	67·31	34·26	70·39
(Y)	20·58	62·47	38·79	32·57	27·55	40·60	27·14	—	55·13	61·49
Z	21·43	64·69	31·51	31·18	30·91	18·60	47·32	70·29	61·42	58·34

Measured by Chron.	7 $0^h\ 22^m$	8 $0^h\ 49^m$	9 $0^h\ 3^m$	10 $3^h\ 56^m$	11 $2^h\ 25^m$	12 $1^h\ 31^m$	13 $0^h\ 15^m$	14 $1^h\ 57^m$	15 $1^h\ 23^m$	16 $2^h\ 37^m$
	s	s	s	s	s	s	s	s	s	s
A	7·74	31·80	39·49	11·67	38·69	33·50	29·40	48·75	67·62	38·96
B	0·51	32·30	37·29	7·07	37·50	27·63	26·30	26·50	67·17	36·30
C	7·31	33·90	39·11	7·53	32·87	33·64	34·31	59·28	69·15	35·05
D	6·86	33·49	39·23	5·43	35·11	31·84	35·28	63·90	69·16	31·87
E	4·42	—	—	—	—	—	—	—	—	—
F	16·60	—	—	—	—	—	—	—	—	—
G	5·90	—	44·81	—	33·99	27·41	30·96	54·15	72·50	17·34
(H)	—	—	—	14·04	35·66	23·94	—	54·31	65·60	—
(K)	—	30·39	36·67	—	38·20	44·60	29·86	42·94	63·09	48·06
L	—	—	38·23	20·65	27·70	32·09	30·91	55·21	83·16	43·18
N	—	36·74	41·69	14·57	40·78	32·52	30·83	57·77	68·64	33·17
O	8·66	33·15	39·36	14·19	34·97	29·29	29·25	51·26	67·72	32·54
P	8·97	—	—	—	—	—	—	—	—	—
R	11·33	32·16	39·40	14·91	36·68	30·69	26·17	42·67	69·53	37·62
S	11·39	32·56	39·44	11·35	28·83	30·17	32·01	52·67	66·55	31·94
(V)	—	—	—	—	27·99	—	38·84	—	—	25·55
W	12·28	35·21	39·19	8·27	32·83	28·52	25·48	36·04	60·06	29·81
X	3·30	29·44	39·28	9·35	28·89	26·82	30·41	47·47	43·24	27·70
Z	9·36	32·90	39·52	16·74	40·48	32·36	32·31	50·96	67·44	34·42

Measured by Chron.	17 2h 36m	18 1h 36m	19 0h 34m	20 1h 36m	v 0h 14m	w 0h 45m	x 0h 14m	y* 1h 32m	z† 0h 16m
	s	s	s	s	s	s	s	s	s
A	25·12	38·39	45·95	27·18	35·82	23·77	50·08	9·76	34·9
B	—	—	44·18	20·25	37·84	28·32	49·39	—	—
C	18·23	31·03	44·96	25·75	36·03	27·40	48·93	8·60	37·8
D	28·16	29·82	45·00	28·31	34·89	27·29	51·46	10·62	42·7
G	32·50	31·46	45·72	24·47	36·80	28·04	52·59	7·15	42·2
(H)	—	—	44·15	23·48	36·76	28 71	50·39	13·06	—
(K)	24·85	43·37	45·42	22·36	37·41	22·70	49·20	—	—
L	23·62	30·34	48·23	28·76	34·85	30·95	51·43	8·82	39·7
N	24·93	27·63	46·37	29·92	35·02	25·03	49·26	8·95	42·1
O	23·46	32·14	45·26	23·56	36·23	28·89	49·88	7·35	43·4
R	21·44	37·70	46·37	26·65	36·19	—	—	—	—
S	18·82	31·00	45·72	24·71	36·79	28·38	48·52	10·29	38·1
(V)	—	29·90	—	—	37·80	29·40	—	10·33	45·5
W	17·74	37·24	45·93	26·28	35·24	27·46	50·43	—	—
X	12·44	—	—	32·50	—	—	—	—	—
Z	19·68	33·96	46·22	26·12	36·97	26·24	50·88	9·52	36·3

* Included: Angra to Falmouth :—

	h	m	s
Chr. A	1	28	38·61
C			39·92
D			41·61
G			37·11
(H)			42·99
L			38·43
N	1	28	40·07
O			38·64
S			41·49
(V)			40·80
Z			38·15

† Included : Portsmouth to Greenwich :—

	h	m	s
Chr. A	0	4	22·35
B			24·57
C			22·63
D			26·73
G			26·46
(H)			29·79
L	0	4	24·07
N			26·63
O			29·17
S			23·29
Z			21·81

By a glance at the foregoing table it is seen that only by four of the chronometers (A, C, D, Z) were all the meridian distances measured, or by five, if S is added to this number, which is missing only in the middle one of the three runs between Bahia and Rio; besides, unbroken chains are presented by W until back to Porto Praya, and by R as far as Pernambuco. Moreover, the measurements by G, L, and N extend from the beginning to the end, but in each of these chains two links are wanting. All the other chains are broken by a greater number of gaps, or extend altogether only from England to the west coast of America. In many instances we cannot but suppose the wanting result to have been suppressed, having been considered untrustworthy on account of its differing from the mean of the others beyond such limits as appeared tolerable, or for too great differences between the rates obtained before and after the run. The great number of breaks in the chains measured by B, (H), (V), and X ought probably to be generally explained in this manner, allowance for which ought to be made, if the relative accuracy of the chronometers is to be judged by the deviations from true or most probable values of the meridional distances, of the separate results *which are given* by Fitz-Roy. On the contrary, that chronometer O is not mentioned in the connection of Monte Video with Valparaiso, evidently comes from quite another circumstance; there cannot be doubt that this chronometer, considered by Fitz-Roy only as rather good (according to my computations one of the very best), has been during that period—and so probably (V) too, in the runs from Port Famine to Valparaiso—on board the tender "Adventure" ("Unicorn"). Of the six chronometers, E, F, M, P, (T), and (Y), entirely wanting in the measurements beyond Callao, four—doubtless E, F, M, and (Y)—were left behind for the completion of the survey of the Peruvian coast by the tender "Constitucion." P is expressly stated not to have been used, because of irregularities of its rate, after September 1835 (from the measurements made with this chronometer, which are given in Fitz-Roy's report, it would appear, however, to have been quite a good instrument), (T) appearing to have been considered as a useless watch since an earlier date. As to L and N lacking in the measurement 7, Valparaiso to Callao, I suppose them to have made this run on board the "Constitucion," instead of E and F, which remained on board the "Beagle" till arrival at Callao. If this supposition is right, there would be in each one of the chains L and N only one measurement, which might appear to have been suppressed for too great a discordance.

Fitz-Roy has adopted as final result for each difference of meridians the simple arithmetical mean from the ten chronometers which gave the most accordant results *inter se* for the difference in question, entirely excluding all the remaining individual results, in some instances the majority of them all. Such a proceeding may give tolerably approximate results in the simplest manner, but

besides being likely to produce, and apparently having produced in this case, illusory ideas on the accuracy of the mean results, it will certainly not afford the most probable values of the single differences; but it cannot by any means be admitted to connect them into a chain, if so doing leads to the consequence that the different links will depend partly on different chronometers, and if at the same time the errors of the chronometrical measurements cannot be regarded as purely accidental; both of which happen to be the case in the present instance.

As a consequence of his method of forming the results for the several links to be joined together, Fitz-Roy has excluded from some meridian distances the very best chronometers, admitting the worst ones with full weight, although these had been excluded for the great majority of the distances. As to systematic errors of the measurements, it ought to be remembered that the great chain Bahia—Bahia proved to be in error by no less than 34^s, an amount the possibility of which Fitz-Roy found himself quite at a loss to understand, as he could not imagine that so large an error could by any means have been produced by the accidental errors of the determinations of local times and of rates, or by the accidental changes of the latter. Granted that Fitz-Roy himself has perhaps considerably underrated the possible effect of these sources of errors, in consequence of dealing only with a selected number of the individual results, they cannot have been in reality so large that any increase, which might be expected from the probability of accidental errors in adding together the twenty links of the chain, could produce a total error comparable with the amount actually found, and Fitz-Roy is very nearly right in saying "it does not appear probable that all the errors would lie in one direction, unless some hitherto undetected cause affects chronometers when carried westward, which might affect them differently when carried eastward." *

Finally, Fitz-Roy has derived the results from his measurements in the following way. To connect Bahia with England, he took the mean of the two results obtained from the outward and the homeward voyage, which were in fair accordance, apparently directly denying the existence of any such "undetected cause," and thereby rendering matters still more unaccountable. From Bahia he simply summed up the measured differences without allowance for the error of the total sum westward to the Galapagos Islands and eastward to New Zealand. Thus the whole amount of error is thrown upon the passage over the breadth of the Pacific, and becomes evident in two values of the longitude of Point Venus being obtained, differing from each other 34^s, of which Fitz-Roy took the mean. How the Hydrographical Office dealt with the error, whether it has tried to arrive at a more reasonable repartition of the whole

* *Narrative*, vol. ii. App. p. 345.

amount, I have no knowledge. In drawing up the French table of positions, Daussy has retained unchanged the longitudes as summed up by Fitz-Roy westward to the Galapagos Islands, and has abstained from making use of the remaining portion of the chain, besides combining for some few eastern places Fitz-Roy's differences with other available determinations. In late editions of the table, the longitudes given by Fitz-Roy along the South American coast have been shifted in groups, according to new French determinations of some of the fundamental meridians.

There is, however, existing an obvious cause which—while affecting the rate of a chronometer, remaining on board in a permanent position, of course always in the same direction—would affect a chronometrical measurement of a difference of meridians in one direction when the chronometer is carried to the west, and in the opposite direction when it is carried towards east. Such a cause is the effect of the motion of the ship. The changes of rates produced by this cause in the case under consideration are to be estimated as much smaller than the changes found in some modern runs with chronometers made by steamships; but we should be very far from right in considering them altogether to be inappreciable in sailing vessels, because the motion of such a vessel also does not consist merely in such vibrations of which the chronometers become independent when swinging freely in their gimbals, but the vessel is also exposed to concussions which will generally communicate themselves to the chronometers—the more so in a small vessel like the "Beagle," measuring only 235 tons—notwithstanding all care in a proper selection and arrangement of their situation on board. It appears at once from a glance at the preceding table, that the better chronometers very regularly differ from each other in the same direction in the measurements of the separate distances, thus directly pointing to regular discordances between sea-rates and harbour-rates of an amount materially different in the different chronometers. For example, A, out of 29 differences of meridians, gives 21 greater (taking the positive sign for western longitudes) than C; O, 15 out of 20 smaller than R; W, 19 out of 26 smaller than Z. Distributing the excess, $33^{s}.67$, of the great chain Bahia—Bahia, by applying simply a correction of $-0^{s}.115 \times$ duration of run (expressed in days) to the results adopted by Fitz-Roy, entirely disregarding for the moment their want of homogeneity, I arrived at the following comparison for the places where the "Beagle's" measurements can now be tested by other determinations :—

Place.	Longitude summing up Fitz-Roy's results westward.	Error.	Correction $-0^{s}.115t$.	Remaining Error.
	h m s	s	s	s
Bahia	2 34 5·42		The starting value	
Rio de Janeiro ...	2 52 36·93	− 1·02	− 0·86	− 1·9
Monte Video ...	3 44 54·53	− 1·90	− 3·27	− 5·2

Place.	Longitude summing up Fitz-Roy's results westward.	Error.	Correction $-0^{s}\cdot115t$.	Remaining Error.
	h m s	s	s	s
Port Famine ...	4 43 53·21	+ 7·91	− 6·50	+1·4
Valparaiso ...	4 46 47·24	+16·40	− 9·65	+6·7
Callao	5 8 56·22	+19·05	−11·38	+7·7
Otaheite	9 58 20·75	+24·85	−16·03	+8·8
Sydney	13 55 27·86	+20·36	−20·52	−0·2
Simon's Bay ...	22 46 53·43	+37·27	−29·95	+7·3
St. Helena ...	0 23 26·74	+33·83	−32·13	+1·7
Bahia	2 34 39·09	+33·67	−33·86	−0·2

The large reduction of errors resulting in all the available comparisons, from so superficial an attempt to correct the measurements as reduced by Fitz-Roy, certainly ought to be regarded as an encouragement to a more exact proceeding, in which the measurements made with each single chronometer were to be treated separately.

As a basis for the necessary corrections, I again took the chain Bahia—Bahia. The measurement of this chain is complete with six chronometers, and with a seventh the chain extends unbroken from Bahia to the Cape of Good Hope. The excess for the first six over 24 hours in 294·5 days of running,* for the last, X, over the true value of the arc Bahia—Simon's Bay in 260·5 days, is as follows :—

s	s	s
A + 55·51	R + 52·59	X − 81·64
C + 10·75	W − 13·72	
D + 31·65	Z + 47·06	

These excesses were distributed among the several links of the chains proportionally to the intervals,† and preliminary means taken for the single links, assuming such relative weights as might be inferred from Fitz-Roy's classification of the chronometers (the pocket chronometers, in the subsequent extension of this operation, being placed in the class next below).‡ After

* For difference 1 the mean of the three measurements has been made use of; in computing the corresponding mean duration of run, the time employed in the second run, made in the direction opposite to that in the first and third runs, ought to be reckoned as negative.

† By inadvertence, proportionally to the intervals given by Fitz-Roy, instead of the mere durations of run, without subtracting from the former the time elapsed between the including observations and the beginning and end of the run respectively. This error, however, cannot have exercised any material influence upon the results.

‡ The weights adopted in this part of the investigation have been :

4 for chr. R
3 for chr. A, W, Z
2 for chr. (K); C, D, E, F, G, L, M, N, O, S
1 for chr. (H), (V), (Y)
0 for chr. (T); B, P.

this it became possible to supply the missing link in chain S, and the two links missing in each one of the chains G, (K), L, and N, by inserting such values as would differ from the mentioned preliminary means by quantities proportionate to the differences in the two or three next preceding and as many of the next following measurements. The excesses of the chains filled up in this manner were then found to be for

G $+44^s\cdot20$	L $+73^s\cdot08$	S $-15^s\cdot46$
(K) $+67^s\cdot89$	N $+63^s\cdot68$	

and were distributed as before. A comparison with a new series of preliminary means formed with the twelve chronometers then afforded the means to annex those chains which were broken by a greater number of gaps, after which definitive means were formed within the reach of the great chain.

Finally, the reductions derived from this chain were used to correct the distances measured from and to England, and, the measurements extending to Greenwich, the longitudes of all the places of observation became determined.

The result of this operation, however, then proved to be unsatisfactory to a most unexpected amount. The outstanding errors for the places of comparison were:—

	s		s
Devonport ...	+0·1	Valparaiso ...	+0·7
Pernambuco ...	−1·6	Callao ...	+1·3
Bahia ...	−3·1	Otaheite ...	+4·0
Rio de Janeiro ...	−5·0	Sydney ...	−4·8
Monte Video ...	−8·8	Simon's Bay ...	+3·9
Port Famine ...	−5·0	St. Helena ...	−1·0

The summing up of the defect to a large amount on the relatively short first track is intolerable, and still more so the rapid change of the error in the short run from Monte Video to Valparaiso; besides, the two results for Bahia coming out 8^s different, there now appeared anything but the harmony Fitz-Roy had found—more accidentally, however, for the mean than actually for the separate chronometers—in the two connections with England.

The impression created by the foregoing table of errors was that the measured meridian distances naturally divided into three groups; the points of separation being Monte Video and Valparaiso, the systematic changes in the rates of the chronometers totally differing from one group to another. After remarking this I searched the "Narrative" for an indication of circumstances which might have caused such a different performance, and then the measurements appeared, as is seen from the dates belonging to each one of them already tabulated above, which are not given in Fitz-Roy's table of the results, actually to

divide in exactly those three sections, separated from each other by long intervals of time, and therefore necessarily to be treated separately. In Section I., Devonport to Monte Video, the measurements have been made in quick succession, very nearly during the first seven months of the year 1832. Then an interruption occurred, extending over almost a year and a half, during which period the oil in the chronometers necessarily changed considerably, and most probably their internal constitution altogether was sensibly altered under the influence of an almost incessant rough shaking, partly in the boisterous sea surrounding Falkland Islands and Tierra del Fuego, partly on board little craft on the worst part of the Patagonian coast. It ought not to be wondered at, therefore, if the chronometers, when the measurements of meridian distances for the chain were resumed in December 1833, were quite different instruments than before, and became influenced by the motion of the ship quite differently than in the voyage from England to Monte Video. Section II., which herewith is to begin, again extends over seven months, comprising only the short run from Monte Video to Valparaiso, but the measurement was interrupted for several months (February to June 1834) by surveying work. After this, again, there occurred an interruption of nearly one year, during which time it is quite reasonable to assume the change in the internal state of the chronometers would have considerably increased; therefore Section III., which extends from the end of June 1835 to the end of October 1836, and contains the measurements carried from Valparaiso at one draught—excepting a somewhat prolonged stay at Callao—to their end at Greenwich, embracing nineteen of the twenty-seven fundamental meridian distances, ought again to be treated separately.

In Section I. the differences

h m s	h m s	h m s
$a+b = 2\ 17\ 24{\cdot}89$,	$1 = 0\ 18\ 32{\cdot}53$,	$2 = 0\ 52\ 18{\cdot}48$*

can be regarded as accurately known. By comparison with these numbers we get therefore several—for most of the chronometers five—equations. How to deduce from them the most probable value of the unknown quantity of the case, the mean difference between sea-rate and harbour-rate, depends upon the assumptions we may make as to the relative amount, in the total differences in question, of the errors of rating, the accidental or systematic variations of the actual value of the mentioned difference, and the accidental changes of rate produced by other causes. I took a middle course, at the same time affording greatest simplicity of computation, between the extreme assumptions which can be made in this respect, simply summing together the

* Instead of this number, $0^h\ 52^m\ 18^s{\cdot}21$ has been used in the computations, assuming Fleuriais' reduction to Rat Island alone, or longitude of observation spot $= 3^h\ 44^m\ 56^s{\cdot}18$, and taking for this value, inadvertently, $56^s{\cdot}16$.

excesses of the measured differences over the three known values of meridian distances—with due regard, of course, to the negative sign of measurement 1^2—and dividing the sum by the sum of the durations of run. In this way the following numbers have been derived:—

Excess of Mean Daily Losing Rate (r_s), the ship being under sail in open sea, over Mean Daily Rate in harbour (r_h), for Section I. (meas. **a, b, 1, 2**)

	s		s		s		s
A	−0·011	G	−0·030	O	+0·127	(V)	−0·058
B	+0·049	(H)	−0·121	P	+0·077	W	+0·093
C	+0·191	(K)	−0·247	R	−0·037	X	+0·098
D	+0·148*	L	−0·007	S	+0·230	(Y)	−0·340
E	−0·041*	M	−0·060	(T)	+0·212	Z	−0·004
F	−0·016	N	−0·078				

It ought to be remarked that the variations of rate in the pocket chronometers scarcely exceed those resulting for many of the box chronometers.

In Section II. the following numbers have been derived for the twelve chronometers used in all measurements belonging to this section, by distributing the error of the whole line † Monte Video—Valparaiso over the 55·5 days of sailing. For the remaining nine chronometers the corresponding numbers have been found by comparing the distances as measured by them with such means as could be formed from the reduced measurements with the twelve chronometers, using preliminary weights, which shall be given hereafter.

Values of ($r_s - r_h$) in Section II. (meas. **3, 4, 5, 6**)

	s		s		s	
A	−0·544	(H)	+0·130	S	+0·115	
B	−1·650 (3 dist.)	(K)	−0·493	(T)	−0·970	very uncertain, 2 dist.
C	−0·100	L	−0·401	(V)	+0·705 (2 dist.)	
D	−0·146	M	−0·265 (3 dist.)	W	−0·225	
E	−0·161 (3 dist.)	N	−0·360 (3 dist.)	X	+0·515	
F	−0·804 (3 dist.)	P	−1·378 (2 dist.)	(Y)	+0·296 (3 dist.)	
G	−0·538	R	−0·575	Z	−0·378	

Some of these values would in any case appear extraordinary for properly mounted box chronometers transported by a sailing

* Allowing for a disturbance, which these two chronometers appear to have suffered in meas. **b**.

† Assuming the longitude of Rat Island as in Section I., and Valparaiso $4^h\,46^m\,29^s{\cdot}96$. I had not then for this place the result of my new reduction of Fleuriais' observations, by combining which with the other available determinations the longitude was considerably increased.

vessel, and the more so when compared with the values occurring in the two other sections. The two greatest values, however, for B and P, are probably in reality chiefly accidental effects of irregular disturbances of rate, by which these two chronometers generally appear to have been affected beyond the average. The same explanation probably holds good for the greatest value next to these, for F, which results mainly from meas. 6; the value given by this chronometer for the preceding meridian distance is nowhere given, whence it ought to be supposed that in the run from Port Famine to Valparaiso its rate was irregular and different from what it was half a year before, when the distances constituting the first half of Section II. were run. The differences $r_s - r_h$ found for the other chronometers I do not consider to be so great that they might not be supposed to originate from the assumed cause; possibly other causes —for example, the variations of temperature—have contributed to them, but that the supposed systematic differences $r_s - r_h$ really have amounted to larger quantities in this section is by no means improbable from the motion of the ship probably being heavier than usual in the other runs; and the very satisfactory agreement of the determination of the middle place, Port Famine, with the true longitude, which will be obtained from the adopted supposition concerning the principal cause of the changes in the rates, is a further strong argument in favour of this supposition.

For the reduction of the last series of the measurements have been employed the following

Values of $r_s - r$ *in Section III. (meas. of diff.* 7–20, v–z).

A	$-0^s.1517$	Valparaiso* – Greenwich
B	$+0^s.1058$	Comp. with prel. means, excl. diff. 14
C	$-0^s.0802$	Valparaiso – Greenwich
D	$-0^s.1114$	,, ,,
G	$-0^s.2166$	Sydney – Greenwich, excl. diff. 16
(H)	$+0^s.0098$	Comp. with prel. means
(K)	$-0^s.1721$	Sydney – Pernambuco
L	$-0^s.2065$	Chatham Island† – Greenwich
N	$-0^s.1624$	Callao† – Greenwich
O	$-0^s.0333$	Valparaiso – Greenwich
R	$-0^s.0836$	Valparaiso – Pernambuco
S	$-0^s.0080$	Valparaiso – Greenwich

* Longitude always as before, $4^h\ 46^m\ 29^s.96$. Allowance for the difference of this number from the most probable value will be made, where necessary, hereafter.

† Using for the longitude of the starting point preliminary means of the results from the other chronometers.

(V)	+0^{s}.1479	Comp. with prel. means
W	+0.0891	Valparaiso—Pernambuco
X	+0.1867	Valparaiso—Simon's Bay, excl. diff. 15.
Z	−0.1299	Valparaiso—Greenwich

The results by E, F, P belonging to this section cannot be used for lack of elements for their reduction, being confined to the first distance only.

The investigation, described above, comprising at once the whole extent of the voyage, had afforded a means to determine the relative accuracy of the different chronometers in a more approximate manner. Comparing the reduced individual measurements with the final results of that investigation I was led to the following classification, differing in some instances from what was previously assumed:—

Weight 4 : Chr. Z
„ 3 : Chr. A, O, N
„ 2 : Chr. C, R
„ 1 : Chr. D, E, G, (H), L, M, S, W

With these weights the "preliminary means" mentioned in Section II. and III., and used for the purpose of determining the values of $r_s - r_h$ for the broken chains, have been deduced.

The weights could be more accurately determined by comparing those meridian distances which had been reduced with the three separate systems of values $r_s - r_h$, with their preliminary means. Excluding some few cases of larger discordances—for certain reasons in each case, but which would be too long to explain in detail—I found for the average value of the probable error of a meridian distance $= \epsilon_1''$, and the corresponding weight p_1 (= 1 for the square of the mean error = 10), the following values, according to which the chronometers would range as below:—

Chron.	ϵ_1''	p_1	Chron.	ϵ_1''	p_1	Chron.	ϵ_1''	p_1
	s			s			s	
O	±1.0	4.15	D	±2.3	0.89	E	±3.1	0.47
Z	1.2	3.00	W	2.3	0.82	M	3.5	0.37
S_3	1.3	2.72	L	2.4	0.78	(K)	4.3	0.22
A	1.7	1.56	G	2.5	0.76	B	5.2	0.17
C	1.9	1.30	X	2.9	0.53	(Y)	6.0	0.13
N	1.9	1.21	(H)	3.0	0.52	$S_{1,2}$	6.3	0.11
P	2.0	1.08	F	3.0	0.49	(T)	10.9	0.04
R	2.2	0.93	(V)	3.1	0.49			

Chronometer S exhibits a great difference in its performance during Section I. and II., and during Section III.; during the first two its rate was very irregular, while in the last it agrees in an admirable manner with the mean of all. To explain this difference we may suppose, that during one of the stays at Valparaiso between Section II. and III. some error of the chronometer was detected and corrected; therefore, it appears

legitimate to use it with different weights in the different periods.

Chronometer O becomes the first of all, when judged from the agreement with the mean of all chronometers, after excluding its result for difference **b**. I consider this proceeding legitimate, because from the discordance of this result the sum of the squares of errors for the remaining twenty-three differences (43·30) would be more than doubled, rising to 131·10, and because, moreover, the result for the first distance, **a**, is not given at all, and therefore cannot but be supposed to have come out quite wrong. Hence it becomes highly probable that at the beginning of the voyage, as far as Porto Praya, some extraordinary cause of error, perhaps only some defect in the mounting, influenced this chronometer, which was then noticed and removed.

The weights of the above table, however, cannot be admitted without further discussion: first, because of their want of homogeneity for the broken chains previously referred to, and secondly, because the relative weights deduced from the above absolute numbers would be erroneous, if there existed sources of error varying from one run to another, of such a nature as would affect the rates of a great number of the chronometers during the same run in the same way. There will be found, indeed, strong indications that a part of the actual errors is due to such causes. I therefore classified the chronometers by the constancy of the observed differences between sea-rates and harbour-rates. This can only be judged from Section III. Dividing the line Valparaiso—Greenwich at four points, viz.: Otaheite, Sydney, Cape of Good Hope, and Bahia, I obtained five determinations of this difference for eight of the twelve chronometers, on which the measurements essentially depend, and four determinations for three of the remaining four, from which resulted the following values of ϵ_2'', or of the probable error of 50 $(r_s - r_h)$, and corresponding weights p_2 ($p_2 = 1$ for a mean error $= \pm 10^s$):

Chron.	ϵ_2'' s	p_2	Chron.	ϵ_2'' s	p_2	Chron.	ϵ_2'' s	p_2
A	±3·4	4·0	O	±6·5	1·1	G	±8·0	0·7
Z	4·0	2·8	W	6·6	1·1	D	9·3	0·5
R	4·1	2·7	N	7·1	0·9	L	12·0	0·3
C	5·9	1·3	S_3	7·3	0·9			

The definitive weights already tabulated at the beginning of this paper, expressed in such round numbers as appeared most plausible, are founded essentially on these two determinations. The better chronometers range according to these weights in the following order: Z, A, O, R, S (in Section III.), C, N, W, G, D, L. Then follow the chronometers used only in a part of the measurements, not differing much from each other in quality, excepting, perhaps, the first of them: P, X, (H), F, (V), E, M; then come (K), B, finally (Y); (T) ought to be rejected.

The meridian distances resulting from the reduction of the measurements by the adopted values of $r_s - r_h$ are contained in the following table:—

TABLE II.

Meridian Distances, corrected for Systematic Change of Rate.

	a	b	1¹	1²	1³	2	3	4	5	6
Measured by Chron.	1h 17m	0h 59m	0h 18m	0h 18m	0h 18m	0h 52m	0h 38m	0h 19m	0h 11m	0h 8m
	s	s	s	s	s	s	s	s	s	s
A	21·46	64·46	32·31	29·64	31·05	15·96	37·63	64·59	51·61	60·04
B	22·78	60·34	—	—	42·99	9·60	—	51·24	59·08	57·71
C	24·51	65·46	32·23	30·45	30·12	13·76	42·38	69·55	44·77	62·87
D	20·09	(43·59)	33·07	30·62	30·33	19·32	47·69	67·42	45·85	67·18
E	19 63	(51·54)	—	27·98	33·78	14·25	47·56	—	48·48	68·11
F	22·71	63·76	35·25	31·96	31·61	14·23	32·44	61·86	—	49·20
G	20·83	65·43	33·39	31·05	30·09	16·94	39·22	59·15	53·63	58·21
(H)	14·74	—	29·53	30·59	—	8·74	45·22	71·14	44·59	67·06
(K)	18·96	73·30	27·05	31·45	25·30	22·17	48·18	56·68	55·48	66·54
L	20·98	61·47	29·64	30·26	31·04	22·74	41·22	65·94	49·96	63·38
M	19·27	58·42	37·21	30·01	—	—	35·51	72·93	47·33	—
N	22·86	62·47	29·95	30·03	32·55	18·15	36·63	—	53·45	62·05
O	—	(52·28)	33·51	30·47	30·89	16·81	—	—	—	—
P	19·27	65·04	33 37	30·95	34·19	14·68	—	—	45·49	58·62
R	19·16	62·78	32·79	31·81	32·68	20·05	35·87	65·88	52·31	60·24
S	25·12	52·84	31·04	—	42 00	17·18	53·87	53·19	48·70	—
(T)	—	45·50	33·03	30·28	33·66	14·34	—	88·40	26·18	—
(V)	21·07	70·07	25·71	30·03	30·57	—	—	—	50·27	63·41
W	22·79	65·68	29·46	32·51	33·15	16·13	41·63	71·74	45·64	65·19
X	23·04	64·05	32·30	31·29	31·04	16·48	48·70	72·98	38·40	66·27
(Y)	13·78	56·01	33·01	33·94	23·81	33·46	32·17	—	55·00	59·12
Z	21·35	64·61	31·44	31·20	30·87	18·52	40·89	66·13	48·15	61·37

Measured by Chron.	7	8	9	10	11	12	13	14	15	16
	$0^h\ 22^m$	$0^h\ 49^m$	$0^h\ 3^m$	$3^h\ 56^m$	$2^h\ 25^m$	$1^h\ 31^m$	$0^h\ 15^m$	$1^h\ 57^m$	$1^h\ 23^m$	$2^h\ 37^m$
	s	s	s	s	s	s	s	s	s	s
A	5·46	30·43	39·19	7·20	34·92	31·38	28·49	46·02	64·74	36·38
B	2·10	33·25	37·50	10·19	40·14	29·11	26·94	(28·40)	69·18	38·10
C	6·11	33·18	38·95	5·17	30·87	32·52	33·83	57·84	67·63	33·69
D	5·19	32·49	39·01	2·14	32·32	30·28	34·61	61·89	67·04	29·98
E	....	—	—	—	—	—	—	—	—	—
F	....	—	—	—	—	—	—	—	—	—
G	2·65	—	44·37	—	28·58	24·38	29·46	50·25	68·39	(13 66)
(H)	—	—	—	14·33	35·90	24·08	—	54·49	65·79	—
(K)	—	28·84	36·33	—	33·90	42·19	28·83	39·84	59·82	45·13
L	—	—	37·82	14·56	22·54	29·20	29·67	51·49	79·24	39·67
N	—	35·28	41·37	9·78	36·72	30·25	29·86	54·85	65·75	30·41
O	8·16	32·85	39·30	13·20	34·14	28·83	29·05	50·66	67·09	31·97
P	....	—	—	—	—	—	—	—	—	—
R	10·08	31·41	39·23	12·44	34·59	29·52	25·67	41·17	67·94	36·20
S	11·27	32·49	39·42	11·11	28·63	30·06	31·96	52·53	66·40	31·80
(V)	—	—	—	—	31·69	—	39·72	—	—	28·06
W	13·62	36·01	39·37	10·90	35·06	29·77	26·02	37·64	61·75	31·32
X	6·11	31·13	39·65	14·89	33·59	29·44	31·54	50·85	(46·81)	30·89
Z	7·41	31·73	39·26	12·91	37·23	30·55	31·53	48·62	64·97	32·21

	17	18	19	20	v	w	x	y	z
Measured by Chron.	$2^h\ 36^m$	$1^h\ 36^m$	$0^h\ 34^m$	$1^h\ 36^m$	$0^h\ 14^m$	$0^h\ 45^m$	$0^h\ 14^m$	$1^h\ 32^m$	$0^h\ 16^m$
	s	s	s	s	s	s	s	s	s
A	21·78	35·51	45·04	25·81	36·81	25·82	47·88	11·13	36·42
B	—	—	44·82	21·20	37·15	26·89	50·92	—	—
C	16·47	29·51	44·48	25·03	36·55	28·48	47·77	9·32	38·60
D	25·71	27·70	44·33	27·31	35·61	28·79	49·84	11·62	43·81
G	27·74	27·35	44·22	22·52	38·21	30·97	49·45	9·10	44·36
(H)	—	—	44·21	23·57	36·70	28·58	50·53	12·96	—
(K)	21·06	40·10	44·39	20·81	38·53	25·02	46·70	—	—
L	19·07	26·42	46·99	26·90	36·19	33·74	48·44	10·68	41·77
N	21·36	24·54	45·40	28·46	36·08	27·22	46·91	10·41	43·72
O	22·73	31·50	45·06	23·26	36·45	29·34	49·40	7·62	43·73
R	19·60	36·11	45·87	25·90	36·73	—	—	—	—
S	18·64	30·85	45·67	24·64	36·84	28·49	48·40	10·36	38·18
(V)	—	32·71	—	—	36·84	27·41	—	9·00	44·02
W	19·70	38·93	46·47	27·18	34·66	26·26	51·72	—	—
X	16·57	—	—	34·19	—	—	—	—	—
Z	16·82	31·49	45·44	24·95	37·82	27·99	49·01	10·75	37·60

The following table contains the means derived for the measured distances from the numbers of the preceding table; that part of the probable error of each mean which can be inferred from the discordances of the individual results; the longitudes (reckoned westward) resulting from summing up the means; the differences of these longitudes from those other determinations which have been referred to at the beginning of this paper.

TABLE III.

Section I.

Assumed Longitude of Starting Point: Devonport = $0^h\ 16^m\ 40^s{\cdot}53$.

Link of Chain.	Mean of Chron. diff. h m s	Weight.	Prob. Error (part.)	Resulting W. Longitudes.	h m s	Error. s
a	1 17 21·22	23	±0·29	Porto Praya	1 34 1·75	
b	1 0 4·19	21	0·38	Bahia	2 34 5·94	+0·52
1^1	0 18 31·98	$25\frac{1}{4}$	0·29			
1^2	30·73	$25\frac{5}{8}$	0·15			
1^3	31·47	25	0·30			
1 (mean)	0 18 31·40	$75\frac{7}{8}$	0·15	Rio de Janeiro	2 52 37·34	−0·61
2	0 52 17·17	25	0·45	Monte Video	3 44 54·51	−1·65

Section II.

Assumed Longitude of Starting Point: Monte Video = $3^h\ 44^m\ 56^s{\cdot}16$ (*Error* $-0^s{\cdot}27$).

3	0 38 39·92	$21\frac{7}{8}$	±0·67	Port Desire	4 23 36·08	
4	0 20 6·21	$19\frac{7}{8}$	0·70	Port Famine	4 43 42·29	−3·0*
5	0 11 49·36	$22\frac{5}{8}$	0·57	Ancud	4 55 31·65	
6	−0 9 1·69	$22\frac{1}{2}$	0·52	Valparaiso	4 46 29·96	−0·88

* Compared with most probable general result: $-1^s{\cdot}79$.

Section III.

Assumed Longitude of Starting Point: Valparaiso = $4^h\ 46^m\ 29^s{\cdot}96$ (*Error* $-0^s{\cdot}88$).

Link of Chain.	Mean of Chron. diff. h m s	Weight.	Prob. Error (part.) s	Resulting W. Longitudes.	h m s	Error. s
7	0 22 7·81	22¼	±0·58	Callao	5 8 37·77	+0·60*
8	0 49 32·31	23	0·32	Chatham Island	5 58 10·08	
9	0 3 39·49	24¾	0·23	Charles Island	6 1 49·57	
10	3 56 10·46	24¼	0·63	Otaheite	9 58 0·03	+4·13†
11	2 25 33·72	25¾	0·57	Paihia	12 23 33·75	
12	1 31 30 04	25¼	0·38	Sydney	13 55 3·79	−3·72‡
13	0 15 29 92	25¼	0·52	Hobart Town	14 10 33·71	
14	1 57 49·10	25	1·10	King George Sound	16 8 22·81	
15	1 24 6·43	24¾	0·54	Keeling Island	17 32 29·24	
16	2 37 33·54	24¼	0·53	Port Louis	20 10 2·78	
17	2 36 20·18	24½	0·58	Simon's Bay	22 46 22·96	+6·80
18	1 16 32·16	24½	0 74	St. Helena	0 22 55·12	+2·21§
19	0 34 45·29	24¾	0·12	Ascension	0 57 40·41	
20	1 36 25·45	25¼	0·36	Bahia	2 34 5·85	+0·43
v	−0 14 36·67	25¼	0·14	Pernambuco	2 19 29·18	+0·22
w	−0 45 27·86	22¼	0·33	Porto Praya	1 34 1·32	
x	0 14 48·80	21¾	0·23	Angra	1 48 50·12	
y	−1 32 10·01	20¼	0·28	Devonport	0 16 40·11	−0·42
z	−0 16 39·76	19¾	0·70	Greenwich	0 0 0·35	+0·35

* Compared with most probable general result: $+0^s{\cdot}52$.
† " " " " $+2^s{\cdot}93$.
‡ Compared with new telegraphic longitude: $-4^s{\cdot}69$.
§ Compared with most probable general result: $+2^s{\cdot}92$.

The deviations of the individual reduced results from these means are contained in the following table :—

TABLE IV.

Differences of the Individual Reduced Measurements from the Mean.

Measured by Chron.	a	b	1^1	1^2	1^3	2	3	4	5	6
	s	s	s	s	s	s	s	s	s	s
A	+0·24	+ 0·27	+0·33	+1·09	− 0·42	− 1·21	− 2·29	− 1·62	+ 2·25	+ 1·65
B	+1·56	− 3·85	—	—	+11·52	− 7·57	—	−14·97	+ 9·72	+ 3·98
C	+3·29	+ 1·27	+0·25	+0·28	− 1·35	− 3·41	+ 2·46	+ 3·34	− 4·59	− 1·18
D	−1·13	(−20·60)	+1·09	+0·11	− 1·14	+ 2·15	+ 7·77	+ 1·21	− 3·51	− 5·49
E	−1·59	(−12·65)	—	+2·75	+ 2·31	− 2·92	+ 7·64	—	− 0·88	− 6·42
F	+1·49	− 0·43	+3·27	−1·23	+ 0·14	− 2·94	− 7·48	− 4·35	—	+12·49
G	−0·39	+ 1·24	+1·41	−0·32	− 1·38	+ 1·77	− 0·70	− 7·06	+ 4·27	+ 3·48
(H)	−6·48	—	−2·45	+0·14	—	− 8·43	+ 5·30	+ 4·93	− 4·77	− 5·37
(K)	−2·26	+ 9·11	−4·93	−0·72	− 6·17	+ 5·00	+ 8·26	− 9·53	+ 6·12	− 4·85
L	−0·24	− 2·72	−2·34	+0·47	− 0·43	+ 5·57	+ 1·30	− 0·27	+ 0·60	− 1·69
M	−1·95	− 5·77	+5·23	+0·72	—	—	− 4·41	+ 6·72	− 2·03	—
N	+1·64	− 1·72	−2·03	+0·70	+ 1·08	+ 0·98	− 3·29	—	+ 4·09	− 0·36
O	—	(−11·91)	+1·53	+0·26	− 0·58	− 0·36	—	—	—	—
P	−1·95	+ 0·85	+1·39	−0·22	+ 2·72	− 2·49	—	—	− 3·87	+ 3·05
R	−2·06	− 1·41	+0·79	−1·08	+ 1·21	+ 2·88	− 4·05	− 0·33	+ 2·95	+ 1·45
S	+3·90	−11·35	−0·94	—	+10·53	+ 0·01	+13·95	−13·02	− 0·66	—
(T)	—	−18·69	+1·05	+0·45	+ 2·19	− 2·83	+22·19	−23·21	—	—
(V)	−0·15	+ 5·88	−6·27	+0·70	− 0·90	—	—	—	+ 0·91	− 1·72
W	+1·57	+ 1·49	−2·52	−1·78	+ 1·68	− 1·04	+ 1·71	+ 5·53	− 3·72	− 3·50
X	+1·82	− 0·14	+0·32	−0·56	− 0·43	− 0·69	+ 8·78	+ 6·77	−10·96	− 4·58
(Y)	−7·44	− 8·18	+1·03	−3·21	− 7·66	+16·29	− 7·75	—	+ 5·60	+ 2·57
Z	+0·13	+ 0·42	−0·54	−0·47	− 0·60	+ 1·35	+ 0·97	− 0·08	− 1·21	+ 0·32

Measured by Chron.	7	8	9	10	11	12	13	14	15	16
	s	s	s	s	s	s	s	s	s	s
A	−2·35	− 1·88	−0·30	−3·26	+ 1·20	+ 1·34	− 1·43	− 3·08	− 1·69	+ 2·84
B	−5·71	+ 0·94	−1·99	−0·27	+ 6·42	− 0·93	− 2·98	(−20·70)	+ 2·75	+ 4·56
C	−1·70	+ 0·87	−0·54	−5·29	− 2·85	+ 2·48	+ 3·91	+ 8·74	+ 1·20	+ 0·15
D	−2·62	+ 0·18	−0·48	− 8·32	− 1·40	+ 0·24	+ 4·69	+12·79	+ 0·61	− 3·56
E	[−3·39]	—	—	—	—	—	—	—	—	—
F	[+8·79]	—	—	—	—	—	—	—	—	—
G	−5·16	—	+4·86	—	− 5·14	− 5·66	− 0·46	+ 1·15	+ 1·96	(−19·88)
(H)	—	—	—	+3·87	+ 2·18	− 5·96	—	+ 5·39	− 0·64	—
(K)	—	− 3·47	−3·16	—	+ 0·18	+12·15	− 1·09	−10·26	− 6·61	+11·59
L	—	—	−1·67	+4·10	−11·18	− 0·86	− 0·25	+ 2·39	+12·81	+ 6·13
N	—	+ 2·97	+1·88	−0·68	+ 3·00	+ 0·21	− 0·06	+ 5·75	− 0 68	− 3·13
O	+0·35	+ 0·54	−0·19	+2·74	+ 0·42	− 1·21	− 0·87	+ 1·56	+ 0·66	− 1·57
P	[+1·16]	—	—	—	—	—	—	—	—	—
R	+2·27	− 0·90	−0·26	+1·98	+ 0·91	− 0·52	− 4·25	− 7·93	+ 1·51	+ 2·66
S	+3·46	+ 0·18	−0·07	+0·65	− 5·09	+ 0·02	+ 2·04	+ 3·43	− 0·03	− 1·74
(V)	—	—	—	—	− 2·03	—	+ 9·80	—	—	− 5·48
W	+5·81	+ 3·70	−0·12	+0·44	+ 1·34	− 0·27	− 3·90	−11·46	− 4·68	− 2·22
X	−1·70	− 1·18	+0·16	+4·43	− 0·13	− 0·60	+ 1·62	+ 1·75	(−19·62)	− 2·65
Z	−0·40	− 0·58	−0·23	+2·45	+ 3·51	+ 0·51	+ 1·61	− 0·48	− 1·46	− 1·33

Measured by Chron.	17	18	19	20	v	w	x	y	z
	s	s	s	s	s	s	s	s	s
A	+1·60	+3·35	−0·25	+0·36	−0·14	+2·04	−0·92	−1·12	+3·34
B	—	—	−0·47	−4·25	−0·48	+0·97	+2·12	—	—
C	−3·71	−2·65	−0·81	−0·42	+0·12	−0·62	−1·03	+0·69	+1·16
D	+5·53	−4·46	−0·96	+1·86	+1·06	−0·93	+1·04	−1·61	−4·05
G	+7·56	−4·81	−1·07	−2·93	−1·54	−3·11	+0·65	+0·91	−4·60
(H)	—	—	−1·08	−1·88	−0·03	−0·72	+1·73	−2·95	—
(K)	+0·88	+7·94	−0·90	−4·64	−1·86	+2·84	−2·10	—	—
L	−1·11	−5·74	+1·70	+1·45	+0·48	−5·88	−0·36	−0·67	−2·01
N	+1·18	−7·62	+0·11	+3·01	+0·59	+0·64	−1·89	−0·40	−3·96
O	+2·55	−0·66	−0·23	−2·19	+0·22	−1·48	+0·60	+2·39	−3·97
R	−0·58	+3·95	+0·58	+0·45	−0·06	—	—	—	—
S	−1·54	−1·31	+0·38	−0·81	−0·17	−0·63	−0·40	−0·35	+1·58
(V)	—	—	—	—	−0·17	−0·47	—	+1·01	−4·26
W	−0·48	+6·77	+1·18	+1·73	+2·01	+1·60	+2·92	—	—
X	−3·61	—	—	+8·74	—	—	—	—	—
Z	−3·36	−0·67	+0·15	−0·50	−1·15	−0·13	+0·21	−0·74	+2·16

In Section I. the distribution of the outstanding errors might be still a little improved; changing, for example, the mean of the adopted values of $r_s - r_h$ by $+0^s{\cdot}01$, the errors would become: Bahia, $+0^s{\cdot}91$; Rio de Janeiro, $-0^s{\cdot}15$; Monte Video, $-0^s{\cdot}98$ (or $-1^s{\cdot}25$, taking the correct mean of Fitz-Roy's and Fleuriais' reduction to the Cathedral). While the smallness of these outstanding absolute errors appears very satisfactory, the difference of the errors for Bahia and Rio, which cannot be diminished by any possible change in the reduction, is somewhat striking, proving the mean of three very nearly agreeing measurements of this meridian distance to be in error by more than a whole second.

In Section II. the reliableness of the reduction can only be tested by the determination of Port Famine. Bearing in mind the limited accuracy of the connection of my new longitude for Punta Arenas with that point—details of which will be found hereafter—the harmony of both results can be regarded as quite satisfactory. On the contrary, the extremely near agreement of all other chronometrical measurements made in this region, by the "Beagle" in her first cruise, during the course of the second expedition by the tender "Adventure," and by Captain Foster in the "Chanticleer," with those results which were adopted by Fitz-Roy,* and the large and regular discordance, therefore, of all of them from my results obtained for Section II., might look astonishing; but it cannot be used as an argument against the correctness of these latter results. All these measurements are entitled only to a small weight; † and the numbers derived from them, together with those given by Fitz-Roy, cannot stand against the fact of their all giving, equally as these latter, the length of the line Monte Video—Valparaiso about 16^s too long.‡

* *Narrative*, vol. ii. App. pp. 348, 349.

† The "Beagle" in her first voyage carried only three chronometers, the "Adventure" (tender 1833–4) probably two. The "Chanticleer" was equipped with a great number of chronometers (17), but their rates were generally irregular, apparently from their being improperly mounted on board.

‡ I will not omit to mention, that another trial to control the reduction of Section II. gave a result at first very perplexing. The longitude of S. Cruz, French Observatory, Transit of *Venus* 1882, according to M. Fleuriais' statements in *Rapports préliminaires*, pp. 61 foll., results:—

	h	m	s
From lunar transits	4	34	10·43
From occultations			12·72
By chronometers (French) from Monte Video ...			12·02
By chronometers (Argentine) from Punta Arenas			12·12

The first of these results, notwithstanding the excellent agreement between the three observers, appears to be some seconds too small, like M. Fleuriais' determinations made 1867–70; the mean of the three other results as they are provisionally given $=4^h\ 34^m\ 12^s{\cdot}29$ certainly can be considered to be a very near approximation to the true longitude. Now the French Expedition has connected the Observatory, among other points, with the American pyramid "at a small distance from Keel Point" and with Mount Entrance, the positions

As to Section III., any considerable improvement of the results arrived at in the preceding investigation could no more be hoped for, as the values of $r_s - r_h$, resulting from the line Valparaiso—Greenwich, were found not differing materially from those applied to the whole chain in the first reduction.

If the correction of $+0^s{\cdot}88$, which the longitude of Valparaiso adopted in the reduction of Section III. appears to require, be duly distributed over the line, the outstanding errors for the places affording other determinations become:—

	s		s		s
Callao ...	+1·43	Simon's Bay ...	+7·09	Pernambuco ...	+0·38
Otaheite ...	+4·82	St. Helena ...	+2·44	Devonport ...	−0·39
Sydney ...	−3·15	Bahia ...	+0·61	Greenwich ...	+0·35

The error does not vanish for Greenwich, because, as may be seen from the table, for some incomplete chains the adopted values of $r_s - r_h$ have been used beyond the ends of the lines from which originally they had been deduced. I have left this error standing as it came out, because at Devonport the error lies in the opposite direction; and, moreover, the measurement

of these points resulting from the above value of the longitude and the latitude given by M. Fleuriais for his Observatory $= -49° 59' 21''{\cdot}5$:—

	°	′	″	°	′	″
American pyramid ...	68	24	43	−50	7	11
Mount Entrance	68	21	31	−50	8	52

Fitz-Roy places in his Table of Positions:

	°	′	″	°	′	″
Keel Point in ...	68	23	30	−50	6	45
Mount Entrance in ...	68	20	0	−50	8	50

His longitudes, therefore, in this region require to be *increased by* 5^s *or* 6^s.

On the contrary, I have derived in the reduction of Section II. the corrections to be applied to his fundamental meridians of Port Desire and Port Famine as $= -4^s{\cdot}72$ and $-9^s{\cdot}21$ respectively, and the mouth of the River S. Cruz being situated nearly midway between these two places, it was to be expected that his longitudes in that region would require a *diminution* equal nearly to the mean of these two corrections, *say* -7^s.

From my new computation, therefore, seemed to arise a difference reaching to the most perplexing amount of 12^s or 13^s; but on closer inspection it became evident that the longitudes of that part of the Patagonian coast depend upon a chronometrical insertion between Port Desire and Port Famine made in the course of the first expedition of the "Adventure" and "Beagle," which appears to have been a failure from the fact of falling short by 9^s of the trigonometrical transport of longitude effected on the same occasion.

This example shows very plainly how precarious it is, as has been done of late, to shift in tracts the shores of South America as laid down by Fitz-Roy, according to new determinations of single points lying within or near the tract. The measurements to the south of a line drawn from the mouth of the River Plate to the Peninsula of Tres Montes are so entangled with each other, that a certain correction of the secondary points from new determinations of the primary points can only be effected by tracing the determination of each place to its very foundation, generally, therefore, only by recurring to the originals of the measurements. Otherwise the changes will remain for this region more or less arbitrary, and more likely to produce confusion than correction.

of the last link of the chain, Devonport—Greenwich, appears subject to an uncertainty considerably above the average.*

The errors in Section III. would, if considered singly, scarcely appear very large, except at the Cape of Good Hope; but, from the rapid variation of the error in the comparatively short distances from Otaheite to Sydney, and from the Cape to Bahia, and from the still larger variation in the line, very long indeed, from Sydney to the Cape, the results cannot but appear very unsatisfactory.

These variations are far greater than could be expected, if the discordances of the chronometers from each other would lead to an approximately true idea of the accuracy of the transports of local time, as is seen by comparing the parts of the probable errors given in Table III. That the chronometers, besides the merely accidental disturbances which alone are shown by these discordances, have been influenced by causes of

* The connections with Falmouth and Portsmouth, included in meas. y and z, do not afford reasons to change the above values.

The difference Falmouth—Angra reduced for 8 days' duration of run becomes from:—

	h m s	s		h m s	s
A	−1 28 39·82, Diff.	+0·26	N	−1 28 41·37, Diff.	−1·29
C	40·56	−0·48	O	38·91	+1·17
D	42·50	−2·42	S	41·55	−1·47
G	38·84	+1·24	(V)	39·62	+0·46
(H)	42·91	−2·83	Z	39·19	+0·89
L	40·08	0·00			

the mean being $= -1^h\ 28^m\ 40^s{\cdot}08$ (weight, $20\frac{1}{4}$). Therefore, from Section III. of the chain the longitude of Falmouth, Pendennis Castle, would be $0^h\ 20^m\ 10^s{\cdot}04$, and the error, compared with the longitude derived from the correction of Mudge's determination, as stated above, $-0^s{\cdot}89$. Dr. Tiarks found by 15 chronometers, 1822, on the outward voyage from Greenwich to Madeira, $20^m\ 11^s{\cdot}49$, and on the return voyage $20^m\ 11^s{\cdot}13$, the mean being $20^m\ 11^s{\cdot}31$, and affording a value apparently very trustworthy, as these voyages have given the longitude of Madeira remarkably correct. Hence the error would be $= -1^s{\cdot}27$; and comparing with the longitude $20^m\ 10^s{\cdot}81$ as resulting from Dr. Tiarks' connection with Dover, effected in 1823 by repeated voyages with 26 chronometers, it would become $= -0^s{\cdot}77$.

The continuation of the chain from Devonport to Portsmouth is possible with only nine chronometers. Assuming the run to have been 2 days, from the numbers given in the foot-note to Table I. the reduced values of the difference, Portsmouth—Devonport, will follow:—

	m s	s		m s	s
A	−12 12·85, Diff.	+1·77	N	−12 15·79, Diff.	−1·17
C	15·23	−0·61	O	14·30	+0·32
D	16·15	−1·53	S	14·83	−0·21
G	16·17	−1·55	Z	14·75	−0·13
L	16·04	−1·42			

mean $= -12^m\ 14^s{\cdot}62$ (weight, $19\frac{1}{4}$), making the longitude of Portsmouth from the chain, Section III., $0^h\ 4^m\ 25^s{\cdot}49$; error, compared with Trig. Survey corr. $= +0^s{\cdot}69$ (compared with N.A. $= +1^s{\cdot}59$). The errors at Falmouth and Portsmouth, therefore, are at all events, as at Devonport and Greenwich, of nearly the same amount with opposite signs.

errors affecting the majority of them, in the same direction, under the same circumstances, and appearing only from a comparison with known longitudes, is shown very plainly by an inspection of those longitudes which would result from each one of the nine best chronometers, forcing them into agreement at the ends of the line with the values $4^h\ 46^m\ 29^s{\cdot}96$ and 0, for the four intermediate points, Otaheite,* Sydney, Simon's Bay, and Bahia. The errors would become at these places from the different chronometers :—

A	C	D	N	O	R	S	W	Z
s	s	s	s	s	s	s	s	s
−4·3	− 3·1	− 7·7	+ 6·4	+ 7·0	+6·6	+7·7	+13·4	+4·8
−9·0	−10·7	−16·1	+ 2·4	− 1·1	−0·3	−4·6	+ 7·2	+1·6
−0·2	+10·1	+14·5	+16·0	+11·8	+1·7	+8·1	− 5·0	+7·1
−3·1	− 0·2	+ 4·5	+ 5·1	+ 2·3	+0·3	+0·0	− 1·7	−0·3

The change of the error therefore is :—

Between	A	C	D	N	O	R	S	W	Z
	s	s	s	s	s	s	s	s	s
Valparaiso & Otaheite	−4·3	− 3·1	− 7·7	+ 6·4†	+ 7·0	+6·6	+ 7·7	+13·4	+4·8
Otaheite & Sydney	−4·7	− 7·6	− 8·4	− 4·0	− 8·1	−6·9	−12·3	− 6·2	−3·2
Sydney & Cape G. H.	+8·8	+20·8	+30·6	+13·6	+12·9	+2·0	+12·7	−12·2	+5·5
Cape G. H. & Bahia	−2·9	−10·3	−10·0	−10·9	− 9·5	−1·4	− 8·1	+ 3·3	−7·4
Bahia & Greenwich	+3·1	+ 0·2	− 4·5	− 5·1	− 2·3		− 0·0		+0·3

The change is greatest in the three middle divisions; and it ought to be remarked that in one of them all the nine chronometers, in the two others eight out of the nine, have changed in the same direction.

Errors in the determination of local time can have contributed but little to these common deviations. The possible influence of such errors upon the measurement of the single differences cannot be made out exactly, because the dates of the four determina-

* In drawing up the above tables, the longitude of Point Venus was assumed, slightly different from the most probable value to be given at the end of this paper, $9^h\ 57^m\ 56^s{\cdot}5$. Allowing for the difference, and increasing the longitude of Valparaiso by the apparently necessary amount, the variations would be smoothed down to some extent between Valparaiso and Sydney—and a little more so by substituting the new telegraphic value of the longitude of the latter place—but, the general character of the second table remaining sensibly the same, I have thought it unnecessary to substitute the later values.

† Between Callao and Otaheite.

tions used in each single measurement are not known, but the following numbers cannot be much in error:—

Corresponding part of probable error of the connection of		Valparaiso and Otaheite ...	$= 3{\cdot}7\,\epsilon''$
,,	,,	Otaheite and Sydney ...	$6{\cdot}3\,\epsilon''$
,,	,,	Sydney and Simon's Bay ...	$8{\cdot}1\,\epsilon''$
,,	,,	Simon's Bay and Bahia ...	$5{\cdot}2\,\epsilon''$
,,	,,	Bahia and Greenwich ...	$9{\cdot}1\,\epsilon''$

ϵ'' denoting the probable error of a complete determination of local time. As ϵ'' will not have exceeded some few tenths of a second, this source of error cannot have produced, except in the last division, any appreciable increase of the actual total error, which becomes evident from the comparison of the longitudes.

The errors of compensation, according to all probability, will have contributed to a certain extent towards the large outstanding differences. These errors, indeed, may be assumed to have been generally similar, especially in the good chronometers, while the variations of temperature happening within the limits of the separate measurements appear from the "Abstract of the Meteorological Journal" to have reached sometimes such amounts as, without assuming extraordinary values of the errors of compensation, might be considered to have produced an appreciable difference of the rate during the run from the mean of the rates determined before and after the run. I have made some trials to arrive, by the aid of the temperatures given in the Abstract, at such determinations of the temperatures of most perfect compensation and of the coefficients of the square, by which the changes of the errors in the five divisions might be reduced as much as possible, but upon the whole without success: on one side because the data of the Abstract proved too inadequate to determine the true temperature of the chronometers; on the other doubtless because errors of another nature have contributed too much to the discordances. From a comparison of the following numbers again the variations of temperature will not appear to have been the principal cause of them:—

Meas.	Interval.	Obs.	$\epsilon'\epsilon'$ for 20 d.	$\tau_h'' - \tau_h'$	$\tau_s - \tau_h'$	$\tau_s - \tau_h''$	Mean of Temp.
	d			°	°	°	°
a	23	4·39	3·82	+24:	+12:	−12	64
b	26	6·78	5·22	+ 6	+ 4	− 2	79
1^1	22	4·59	4·17	− 6	0	+ 6	80
1^2	6	1·27	4·23	+ 4	0	− 4	77
1^3	14	5·08	7·26	− 6	− 4	+ 2	77
2	2	10·94	9·12	−16	− 8	+ 8	66

Meas.	Interval.	Obs. $\varepsilon\varepsilon'$	for 20 d.	$\tau_h''-\tau_h'$	$\tau_s-\tau_h'$	$\tau_s-\tau_h''$	Mean of Temp.
	d			°	°	°	°
3	19	21·81	22·96	− 8	− 7	+ 1	64
4	16	21·16	26·45	− 7	− 2	+ 5	58
5	27	16·10	11·93	+ 7	+ 6	− 1	47
6	12	13·26	22·10	+ 6	+ 5	− 1	54
7	25	16·20	12·96	+ 7	+ 5	− 2	62
8	12	5·27	8·78	+ 7	+ 4	− 3	69
9	4	2·79	13·95	—	—	—	72
10	31	21·48	13·86	+ 8	+ 5	− 3	76
11	28	18·61	13·29	−11	− 5	+ 6	75
12	19	8·12	8·55	+ 4	+ 2	− 2	71
13	11	15·05	27·36	− 6	− 6	0	69
14	20	66·87	66·87	+ 1	− 3	− 4	66
15	20	15·76	15·76	+14	+ 6	− 8	75
16	21	14·95	14·24	− 3	− 1	+ 2	81
17	25	17·16	13·73	−15	− 6	+ 9	72
18	21	29·31	27·91	+ 7	+ 3	− 4	68
19	7	0·79	2·26	+ 5	+ 2	− 3	73
20	10	7·13	14·26	+ 2	+ 1	− 1	77
v	7	1·12	3·19	0	0	0	78
w	14	5·28	7·54	+ 3	+ 3	0	80
x	15	2·55	3·40	− 6	− 3	+ 3	78
y	14	3·44	4·42	−16	− 9	+ 7	67
z	11	21·18	38·50	− 6 :	+ 1	+ 7 :	57

Here "$\varepsilon'\varepsilon'$ obs." is the square of the mean error for weight 1, resulting, for each single difference of meridians, from the deviations of the individual results from their mean, and this value has been reduced in the next column to an interval of twenty days, assuming $\varepsilon'\varepsilon'$ to vary proportionally to the length of the intervals. The last four columns contain the differences between the mean temperatures—in the meaning explained before—in which the chronometers were rated (τ_h' and τ_h'') and transported (τ_s), and the means of these three temperatures.

Two values of $\varepsilon'\varepsilon'$ reduced to the unit of time appear most extraordinarily large. They belong to those two measurements in which τ_s has not been, as in all the other measurements, included between the limits τ_h' and τ_h'', but was outside of them. This circumstance indeed would seem to point to errors of compensation being the cause, and really such errors may be the principal cause of the unusual want of harmony between the individual results in the last measurement, as possibly τ_h''

has in this case been in reality much lower than assumed in the table.* The abnormal discrepancy of the chronometers in the passage from Hobart Town to King George Sound (meas. 14) must have been produced by other, unknown, circumstances; and besides these two instances there appears nowhere from the table any indication of a connection between the amount of the discordances, and the variations, or the absolute amount of temperature.

The effects of temperature, therefore, which to a certain extent may have contributed towards the outstanding errors, appear to be concealed by larger errors of a different kind. Indeed the assumption upon which the computation of Section III. has been founded, though indispensable, is necessarily erroneous, because the chronometers (which moreover had not been cleaned for four years) would not in reality maintain a uniform performance through so long a period as seventeen months, and under the varying circumstances of the different runs. It will chiefly be the errors of this assumption, viz., the actual variations of $r_s - r_h$, which show themselves in the outstanding errors of the longitudes; and as to the remarkable general agreement of these errors, and, correspondingly, of the variations of $r_s - r_h$ for the different chronometers, it might seem *a priori* quite probable, that the amount and character of the ship's motion under the influence of wind and sea would have influenced the great majority of the chronometers in the same direction.

Possibly also an influence of terrestrial magnetism has contributed to the irregularities occurring within the chain.† It is not possible to examine this closely, because no statements are given as to the ship's position during the rating of the chronometers.

For the longitudes independent determinations cannot be nearer approximated from the published data, unfortunately incomplete in so many respects, relating to the "Beagle's" measurements, than has been done in Table III. Therefore the present investigation might be looked upon to a certain degree less as a matter of practical value than of personal interest in an undertaking which, in my opinion, has been most admirably managed both in its nautical and scientific aspects; but I consider such results entitled to present value, as will become available from this investigation, for determining longitudes not yet settled in a more unobjectionable way, by interpolating the error of the chain between the nearest well-known places. The longitudes resulting from the "Beagle's" measurements are now entirely superseded for Pernambuco, Bahia, Rio de Janeiro, Monte Video,

* See *Abstract of Met. Obs.* p. 62.

† A chronometer, which recently passed through my hands, of very superior quality in other respects, had its daily rate changing to an amount of nearly 3^s, according to the magnetic azimuth in which a certain diameter was placed.

Valparaiso, Sydney, and the Cape of Good Hope. It will be the same with Callao, Hobart Town, and Port Louis (Mauritius), when the results of the determinations of the longitudes of these places, recently made, will be known. The "Beagle's" longitudes are entitled still to contribute, with some weight, to the determination of Port Famine, and of one place of considerable astronomical interest, Point Venus; for the Azores group, for Porto Praya, the two astronomically interesting islands, St. Helena and Ascension, further for Port Desire, Ancud, the Galapagos group, Paihia, King George Sound, and Keeling Islands, they still afford the most accurate results.

The following is a summary of those of the "Beagle's" results which may be considered to have practical value in the present day:—

Porto Praya, Island of St. Jago.

	h	m	s	
Longitude found in Section I.	1	34	1·75	W.
Correction interpolated between Devonport and Bahia			−0·24	
Longitude corrected	1	34	1·51	W.
Longitude found in Section III.	1	34	1·32	W.
Correction interpolated between Pernambuco and Devonport			−0·01	
Longitude corrected	1	34	1·31	W.
Mean of the two determinations	= 1	34	1·41	W.

Angra, Terceira.

Longitude from Section III.	1	48	50·12	W.
Correction interpolated from Porto Praya (mean as before) and Devonport			+0·26	
Longitude corrected	1	48	50·38	W.

St. Helena.

Longitude from Section III.	0	22	55·12	W.
Correction interpolated between Simon's Bay and Bahia			−3·28	
Longitude corrected	0	22	51·84	W.
Johnson's longitude, as corrected above, is ...	0	22	52·91	W.
The most probable result will at present be, allowing weights as 2 : 1	0	22	52·20	W.

Ascension.

Longitude from Section III.	0	57	40·41	W.
Correction interpolated as for St. Helena ...			−2·11	
Longitude corrected	0	57	38·30	W.

Port Desire.

	h	m	s	
Longitude from Section II.	4	23	36·08	W.
Correction interpolated from Monte Video (+0ˢ·27) and Port Famine (see below) ...			+1·10	
Longitude corrected	4	23	37·18	W.

S. Carlos de Ancud, Chiloë.

Longitude from Section II.	4	55	31·65	W.
Correction interpolated from Port Famine and Valparaiso (+0ˢ·88)			+1·16	
Longitude corrected	4	55	32·81	W.

Chatham Island, Galapagos.

Longitude from Section III.	5	58	10·08	W.
Correction interpolated from Callao (see below) and Point Venus (see below)			−1·13	
Longitude corrected	5	58	8·95	W.

Charles Island, Galapagos.

Longitude from Section III.	6	1	49·57	W.
Correction interpolated as before			−1·34	
Longitude corrected	6	1	48·23	W.

Paihia, Bay of Islands, New Zealand.

Longitude from Section III.	11	36	26·25	E.
Correction interpolated from Point Venus and Sydney			−1·03	
Longitude corrected	11	36	25·22	E.*

King George Sound.

Longitude from Section III.	7	51	37·19	E.
Correction interpolated from Sydney and Simon's Bay			−0·35	
Longitude corrected	7	51	36·84	E.*

Keeling Islands.

Longitude from Section III.	6	27	30·76	E.
Correction interpolated as before			+1·82	
Longitude corrected	6	27	32·58	E.*

The last two results are open to improvement, as soon as the correction can be derived from the new determinations of Hobart

Town and Port Louis. From the "Beagle's" measurements the longitudes of these places would be, interpolating the correction to be applied to the values derived in Section III. from Sydney and Simon's Bay:—

	h m s	s	h m s
Hobart Town ...	9 49 26·29	−2·52	=9 49 23·77 E.*
Port Louis ...	3 49 57·22	+4·09	=3 50 1·31 E.*

Finally we have, combining the "Beagle's" results with values already used in the preceding investigation:—

Port Famine.

	h m s
Longitude from Section II., interpolating between corrections +0ˢ·27 for Monte Video and +0ˢ·88 for Valparaiso (+0ˢ·56)	4 43 42·85 W.
By connections with Punta Arenas (see note)	4 43 45·3 W.
Probable mean	4 43 44·08 W.

Callao.

	h m s
Longitude from Section III.	5 8 37·77 W.
Correction interpolated from Valparaiso (+0ˢ·88) and Point Venus (−2ˢ·93 according to following determination) ...	−0·44
Longitude corrected	5 8 37·33 W.
From telegraphic connection with Pisco ...	5 8 37·17
Probable general result, mean	5 8 37·25 W.

Point Venus, Otaheite.

	h m s
Longitude from Section III., allowing for the correction to Valparaiso	9 58 0·72 W.
Mean of three determinations given above	9 57 55·9
Probable general result, allowing weights as 1 : 3	9 57 57·10 W.

In his summary of the results for the great chain, vol. ii. App. pp. 347–350, Fitz-Roy has given determinations, besides those for the places the longitudes of which have been anew deduced in the preceding investigation, for Fernando Noronha, Port Louis (Falkland Islands) and Cape Horn, but without giving any details of these determinations. The last two places can only be supposed to have been determined when the "Beagle" sailed from Port Famine to Berkeley Sound in February and March 1834, therefore belonging to Section II., whence it would

* Substituting the new determination for Sydney, the corrections would be interpolated as −1ˢ·61, −1ˢ·02, +1ˢ·35, −3ˢ·39, +3ˢ·82, and the resulting longitudes of the places depending upon Sydney would become: Paihia 11ʰ 36ᵐ 24ˢ·44 E., King George Sound 7ʰ 51ᵐ 36ˢ·17 E., Keeling Islands 6ʰ 27ᵐ 32ˢ·11 E., Hobart Town 9ʰ 49ᵐ 22ˢ·90 E,, Port Louis 3ʰ 50ᵐ 1ˢ·04 E.

be inferred, that to both places considerably too great longitudes have been assigned.

Fernando Noronha was determined in the passage from Porto Praya to Bahia, 1832. I may add some few words to establish the most probable result of this determination and of some others, which are available to fix the longitude of this island, which, both by position and shape, is so admirably suited to rate chronometers on board ships traversing the equatorial part of the Atlantic Ocean.

The means, alone given by Fitz-Roy, for the distances of Fernando Noronha from the two including meridians (ends of arc **b**) most probably depend exclusively upon the same ten chronometers, from which, as from the ten best agreeing, he derived his final result for arc **b**; and further it is probable that the numbers originally given by their mean have been:—

Meridian distance from Porto Praya to Fernando Noronha (observations, 1832 Feb. 5 and Feb. 20, run 12 days) $35^m\ 39^s{\cdot}6$.

Meridian distance from Fernando Noronha to Bahia (observations, 1832 Feb. 20 and March 2, run 7 days) $24^m\ 23^s{\cdot}4$.

These values ought to be reduced by the mean of the values of $r_s - r_h$ found in Section I. for the ten chronometers $= -0^s{\cdot}0094$, whence the longitude becomes

	h	m	s	m	s	h	m	s
From Porto Praya ...	1	34	1·40	+35	39·49	=2	9	40·89
From Bahia	2	34	5·42	−24	23·33	=2	9	42·09

the mean, with weights as 11 : 15, being $2^h\ 9^m\ 41^s{\cdot}58$. The observations are said to have been made as nearly as possible at the same spot as those of Foster,* the reduction to the pyramid therefore is to be assumed $= +2^s{\cdot}22$, and the longitude of this point becomes $2^h\ 9^m\ 43^s{\cdot}80$.

Two more determinations can be considered as equally trustworthy: one made by Lee (cruise of the "Dolphin" 1851–2), $9^m\ 47^s{\cdot}27$ E. of Pernambuco, Fort Picão,† making the longitude $= 2^h\ 9^m\ 40^s{\cdot}50$; the other by Mouchez, $42^m\ 56^s{\cdot}53$ E. of Rio de Janeiro, Fort Villegagnon,‡ making the longitude $= 2^h\ 9^m\ 41^s{\cdot}42$. The mean of the three determinations becomes:—

	h	m	s
Fernando Noronha, Pyramid,	2	9	41·91 W.

Note.

The amount of uncertainty in the connection of Port Famine with the German Observatory at Punta Arenas will appear from the following data. When the "Beagle" surveyed the Straits, the settlement at Punta Arenas did not yet exist, being established at this point now only thirty years ago. The nearest points laid

* *Narrative*, vol. ii. p. 59. *Voyage of H.M. Sloop "Chanticleer,"* vol. ii. p. 238.

† *Conn. d. T.* 1858, Add. p. 60; 1873, Add. p. 18.

‡ *Ib.* 1870, Add. p. 86.

down in the Survey have been "Sandy Point extremity," the end of the broad flat point which has given its name to the settlement, lying $2\frac{1}{2}$ nautical miles to the N.E.—while Port Famine is nearly 30 miles distant to the south of the settlement —and Cape Negro, distant 14 miles towards the north. Now the positions assigned to these two points in the tables given as results of the expeditions of the "Adventure" and "Beagle" do not agree, King making them respectively 4′ 30″ and 8′ 31″ to the E. of Port Famine,* Fitz-Roy 5′ 45″ and 8′ 45″ E.† As the "Table of Positions" given by Fitz-Roy would appear to contain the combined results of both expeditions, we might assume the latter values to be more accurate, and the first determination of Sandy Point extremity to be affected by some error. But in 1867 M. Fleuriais connected both these points by a triangulation with his Observatory—situated almost exactly on the same spot as that occupied by the German Observatory 1882—and found the difference of their longitudes almost exactly agreeing with King, placing "Sandy Point extremity" 2′ 49″·8, Cape Negro 6′ 47″ to the E. of his Observatory.‡ Applying the difference: German Observatory W. of French transit $0^{s}{\cdot}01$, Port Famine therefore would be found W. of German Observatory, Punta Arenas :—

	s
From Sandy Point extremity, by Fleuriais and King ...	6·67
„ „ „ Fitz-Roy ...	11·67
From Cape Negro, by Fleuriais and King	6·94
„ „ Fitz-Roy	7·87

The difference again existing between Fitz-Roy and Fleuriais, or part of it, cannot be explained by assuming the flat to have considerably changed in the interval, as from the locality the change would probably happen in the opposite direction; and I should be inclined to explain the differences between all the three determinations by the indistinctness of the point to be observed in the case of "Sandy Point extremity," the different observers being likely to have pointed, from a distance, at largely different spots of the broad flat "Point."§ On the contrary, Cape Negro is a good mark, and the possible doubt as to the identity of the objects pointed at would seem to be confined within the limits of some few seconds of arc on the earth's surface.‖ I should not hesitate, therefore, if these

* *Narrative*, vol. i. p. 481.

† *Ib.*, vol. ii. App. pp. 72, 74, 76.

‡ *Conn. d. T.* 1871, Add. p. 21.

§ Fleuriais' measurements differ also in the latitude of Sandy Point extremity considerably—more than 1′—from the two English determinations, which agree in this coordinate, while in the latitude of Cape Negro all the three determinations agree to a few seconds.

‖ The limits would be so narrow, if M. Fleuriais by "Cape Negro" means at all the same object as King and Fitz-Roy, viz. the N.E. point of Laredo Bay. The *highest* point, however, of the ridge forming the N. shore of Laredo Bay is situated considerably (nearly 1′) towards W.

measurements alone were available, to take for each point the mean of King's and Fitz-Roy's differences, allowing double weight to the reduction from the more distant but better point.

Another determination, however, can be made by means of one of the very best landmarks in the Straits, Mount Tarn. From some angles taken in the vicinity of the German Observatory, the true bearing of Mount Tarn—peak at north end—from the transit pier, = S. 6° 45′ 25″ W. Mount Tarn, from the fact of being only eight nautical miles distant from Port Famine, and allowing of a very distinct pointing, would appear to be very accurately placed relatively to this latter point by the differences resulting from the table of positions: long. 4′ 25″ W., lat. 6′ 51″ S. The absolute value of the latitude of Port Famine appearing very accurate too, the resulting latitude of Mount Tarn = −53° 45′ 6″ compared with the latitude of the German Observatory = −53° 9′ 39″·35 can be safely used to derive from the observed azimuth the difference of longitude, which becomes 7′ 6″·14, whence Port Famine would be 2′ 41″·14 = 10ˢ·74 W. of Punta Arenas, sufficiently agreeing with the reduction from Fitz-Roy's determination of Sandy Point extremity, but widely inconsistent with King's, and with the result from the apparently better of the two northern points.

In this confused state of things, I can at present only take a simple mean from the determinations by the three different points, using for the two northern the means from King and Fitz-Roy. Then Port Famine becomes W. of Punta Arenas from Sandy Point extremity, 9ˢ·17; from Cape Negro, 7ˢ·40; from Mount Tarn, 10ˢ·74; mean = 9ˢ·10; whence the longitude of Port Famine is found, as made use of in the comparisons of the preceding investigation, = 4ʰ 43ᵐ 45ˢ·3 from a result for the longitude of the German Observatory, which is not yet the definitive, but cannot differ from the definitive by more than a few tenths of a second.

Berlin: March 1884.

Errata.

Page 228, third line from top, *for* $s = a + \left(1 + \frac{n-n_0}{n^\circ}\right).t + (n-n_0).t$,

read $s = a + 24^h \left(1 + \frac{n-n_0}{n^\circ}\right).t + (n-n_0).t$.

„ 231, seventh line from top, *for* $= t'\left(1 + \frac{\delta n}{360^\circ + n}\right)$,

read $t = t'\left(1 + \frac{\delta n}{360^\circ + n}\right)$.

„ 232, second line from top, *for* $s = a + 24^h\, t_0 + \frac{n+\delta n}{15}.t$,

read $s = a + 24^h\, t_0 + \frac{n+\delta n}{15}.t'$.